N H
Declass 45

133p

NATIONAL AERONAUTICS AND SPACE ADMINISTRATION,

WASHINGTON, D. C.

ORBITING ASTRONOMICAL OBSERVATORIES
PROJECT BRIEFING

NASA TM X-50191

Auditorium
NATIONAL AERONAUTICS AND SPACE ADMINISTRATION
1520 H Street, N. W.
Washington, D. C.

9:00 a.m.
Tuesday, December 1, 1959

DR. NANCY G. ROMAN, Head, Observational Astronomy Program, Office of Space Sciences, Presiding.

Table of Contents

	Page
Introduction	2
Ultraviolet Stellar Spectrograph (Goddard Space Flight Center)	6
Discussion	13
Ultraviolet Photometer (University of Wisconsin)	16
Discussion	23
Ultraviolet Sky Mapping (Smithsonian Astrophysical Observatory)	32
Discussion	40
High Dispersion Stellar Spectrograph (Princeton University)	45
Discussion	53
Solar Experiments (University of Michigan)	59
Discussion	66
General discussion of experiments	70
Engineering Aspects - Mr. Triplett and Mr. Foster	78
Kitt Peak National Observatory - Advanced Planning	119
Concluding questions and remarks	124
Figure	130

TMX 50,191

AGENDA

Orbiting Astronomical Observatories Project Briefing
9:00 a.m. 1 Dec 1959
Auditorium
National Aeronautics and Space Administration
1520 "H" Street, N. W.
Washington, D. C.

Presentations --

1. Basic Philosophy of the Orbiting Astronomical Observatories Project
   NASA Astronomy and Astrophysics Programs

2. Ultraviolet Stellar Spectrograph
   NASA Goddard Space Flight Center

3. Ultraviolet Photometer
   University of Wisconsin

4. Ultraviolet Sky Mapping
   Smithsonian Astrophysical Observatory

5. High Dispersion Stellar Spectrograph
   Princeton University

6. Solar Experiments
   University of Michigan

7. Engineering Aspects of the Orbiting Astronomical Observatories
   NASA Ames Research Center

Brief Recess

Discussion Period

Preface

This transcript is an essentially unedited version of the proceedings of a meeting held to acquaint the industrial community with the technical aspects of the Orbiting Astronomical Observatories Project.

It should be understood that this was not a proposers, bidders, or manufacturers' conference, but purely a meeting whereby NASA could provide additional information to those companies which have shown a continuing interest in this project and particularly answer the numerous questions which have arisen.

For clarification of statements made at this meeting it is requested that you contact the speakers directly.

Nancy G. Roman

PROCEEDINGS

DR. SCHILLING: Good morning, ladies and gentlemen. It is my pleasure to welcome you to this briefing session on our orbiting astronomical observatories projects. Each of you has received our invitation and a tentative agenda. We are not ready yet to let out formal invitations for bidding on this project. Rather, we have experienced since the last few months that many of you have become so interested in this project that we have had with us just about everyone in this room, and I think many of you have visited some of our contractors who are merely engaged in preliminary instrumentation and scientific development.

We thought the easiest way for you, as well as for us, would be to present our present plans, and the present status of this project in a general session like this one. The Orbiting Astronomical Project is one of the projects in our observational astronomy program. The head of this observational astronomy program is Dr. Nancy Roman, who will lead this session today, and present to you the various speakers assembled at this table.

I want to introduce to you at this time Dr. Nancy Roman.

DR. ROMAN: Thank you. I think I had better stay seated. Astronomers have for a long time wanted to get beyond the earth's atmosphere. The earth's atmosphere has two very serious effects. First, the light which does get through the atmosphere is distorted, and secondly, much of the radiation does not get through because of molecular absorption in the earth's atmosphere.

Balloons and now satellites give us the first opportunity to get beyond the atmosphere and study radiation from celestial sources in an undistorted form. However, all astronomical sources with the exception of the sun are faint, and almost all, because they are very distant, are extremely small angularly. Therefore, you can not exploit the advantages of getting above the atmosphere unless you are able to get up there reasonably large sized telescopes, and unless you are able to keep these telescopes pointing at one region of the sky for long periods of time to a high degree of accuracy.

It was with these considerations in mind that we started the Orbiting Astronomical Observatories Project. The purpose of this project is to satisfy these requirements, that is, to get a telescope of moderate size, up to perhaps 36 or 40 inches in diameter, in a satellite, which can be pointed to within a fraction of a second of arc for periods as long as the star remains visible, which for the type of orbit we are considering is about 45 minutes to an hour in most positions.

Now, it is obviously a fairly large undertaking to produce such a satellite. It is also obvious that these basic requirements are necessary for almost any type of astronomical experiment which we wish to conduct in the region in which optics are used. Therefore, we have planned the satellite for this project with the idea that we would have a basic shell, if you want to call it that, which we have been calling our stabilized platform system, into which various types of optical instrumentation could be inserted. We have divided the responsibility on this project. NASA is directly taking over the responsibility for the stabilized platform system which will provide all of the features common to any astronomical experiment which we would like to do. This will include the stabilization, guidance, power supply, and some sort of finder system to tell where the telescope is pointing.

In addition to this, responsibility for individual experiments has been distributed among university and other scientific groups who are represented here today.

I think at this point I would like to go around the table and introduce the people whom you see here. Starting from this end, we have Dr. Code, Washburn Observatory, University of Wisconsin. Mr. Triplett, Ames Research Center, an NASA laboratory. Mr. Robert Davis, Smithsonian Astrophysical Observatory. Dr. Kupperian of Goddard Space Flight Center of the NASA. Mr. James Milligan, Goddard Space Flight Center. Dr. J. Rogerson, Princeton. Dr.

Liller of the University of Michigan Observatory. You will hear from each of these gentlemen in just a minute.

I think most of you have received the preliminary specifications which were prepared at the Ames Research Center outlining the general requirements for this project. If any of the companies here in the audience have not received these specifications, if you will please give me a slip of paper with your name and address, I will see to it that you get a copy within a week or so. I do not have any to distribute this morning, unfortunately.

Dr. Schilling has already outlined the purpose of this meeting. I think you all have the agenda in hand. The idea is that each of the astronomical groups represented will give a short description of their experiment. I will then invite questions from the audience on that particular experiment. At the end we will have a discussion of some of the engineering aspects of the project by the Ames Research Center, and then we will throw the meeting open to general questions on any portion of the project.
I hope to be able to finish the major portion of the meeting by one o'clock, but if there is sufficient interest to continue into the afternoon, this will be possible.

There is one other point which perhaps I should mention at this time. The preliminary specifications were written in terms of the Vega vehicle. It appears now as if the Vega may be eliminated, and instead an Atlas Agena B substituted. This is a very new development. I don't think it will affect the program markedly. I have not had the time to go into the capabilities of the Agena in detail. This is a very new development, as I say, and it still is not firm. However, it looks like it should be able to do about the same job as the Vega.

With this I will turn the program over to the astronomers -- I will take it back. There is one other person in the audience I would like to introduce at this time. This is Mr. Curran of our Procurement Division. Mr. Curran, will you please stand? If any of you have detailed questions on procurement procedures, as compared to technical questions on this project, I would appreciate it if you would direct it to Mr. Curran.

Now I would like to turn the meeting over to Mr. Milligan, who will discuss the Goddard experiment for an ultraviolet stellar spectrograph.

ULTRAVIOLET STELLAR SPECTROGRAPH, NASA GODDARD SPACE FLIGHT CENTER, BY J. MILLIGAN

MR. MILLIGAN: I would like to spend a few minutes sketching out to you a tentative optical design for an astronomical telescope. I will spend very little time, if any, on techniques of guidance, or things of this sort. I will leave that for questions later on.

(Blackboard demonstration.) (See Figure)

MR. MILLIGAN: The telescope that we are envisioning will probably take the form of something of the sort I am sketching out on the board. At least it will be able to fit into the overall constraints that I am giving you. The telescope will consist, that is, the collecting optics will consist of a 36 inch mirror which will be operating probably around F-1, and a secondary which will put a return beam through the hole in the primary mirror of about F-5. Since this is a 36 inch mirror, this distance from here to here will be of the order of somewhere between 30 and 36 inches, something of that order of magnitude. The light will then come through a diaphragm here on a collimating mirror, back into the telescope, on to a large grating, on to a camera here, over to a detector system.

Now, this system is being designed for spectrometry. We are asking for moderate to low resolution. We are more interested in photometry than we are in high resolution work. We are talking in terms of the spectral resolutions varying from one angstrom, which will be our best resolution, to resolutions of the order of 50 angstroms. This telescope is being designed for two purposes. One, to get absolute energy distributions of stars in the wavelength regions from 4,000 angstroms to 1,000 angstroms. The second thing that we want to use it for, we want to get spectra on an absolute basis of emission nebulae which occur in the sky and integrated stellar systems, that is, galaxies. So we have essentially two different purposes. In one case, we are dealing with a point source in the sky. In the other case we are dealing with an extended region.

Our guidance accuracies in the two cases will be a little bit different, say from the point source here for the extended region, called No. 2. From the point source it looks as though we are going to need guidance accuracy of the order of one second of arc for periods of the order of 40 seconds. That is, we want the drift within a period of 40 seconds to be no greaterthan one second of arc. On the extended emission regions we will be very happy if we can get guiding accuracies of the order of one minute of arc for a period of, let us say, 25 minutes.

At the present time the plan is to use one or more photomultipliers as a detection system. These photomultipliers will have slits in the front of them, and each one of them will be looking at a certain wavelength band. To produce scanning of the spectrum in the observatory, it looks as though we are going to have to rotate this grating in finite steps every 20 seconds, something of this sort. That is one reason I gave guidance of one second accuracy in the period of the order of 40 seconds.

The detectors will be used as photon counters and the readout system will be something as follows. We want to have data of the order of one per cent accuracy. This is very, very difficult to do in a telemetry system such as we are probably going to be dealing with in a satellite. The way we are going to be able to do this, we want to be able to count these photons in each one of the detectors so that photon-wise we will have accuracies of the order of one per cent with a final accuracy of .1 per cent. The way we are planning on doing this at the present moment is to essentially count on each one of these detectors

and measure the time interval it takes for us to count a certain number of counts.

We will then store the time it takes for the detector to count the counts, and telemeter the information back to the ground. So essentially what we will be telemetering back is not the number of counts that we receive, but the number of counts which we feel is necessary to produce the statistical accuracy that we need, and we are telemetering this information back in terms of a time system, that is telemetering back the time it takes to develop these numbers of counts.

DR. MEINEL (Kitt Peak National Observatory): That is only one per cent basic accuracy.

MR. MILLIGAN: I said to the order -- this number is a free-parameter at the present moment.

DR. MEINEL: Ten to the sixth?

MR. MILLIGAN: This number is only a free-parameter at the moment. This means that as we scan a stellar spectrum, we are going to have to have storage of our data. We feel that this is part of our experiment so that we will handle this ourselves. We will supply a data storage system and make it compatible with the telemetering system.

At the present moment it looks as though we are going to have to go to a core type memory instead of a tape recorder. As far as telemetry goes, it looks as though the basic experiment will be easily handled by two channels of information. One, for example, could be an FM, which is not at all definite, an FM system, let us say, with a 1 KC band width. The second one will be a lower frequency channel, let us say of the order of a tenth of a KC band, which we will commutate to read back information such as characteristics of the detectors, voltage, and things of that sort. That is, environment type devices.

As far as the accuracy goes, this is all the telemetering requirements we see at the moment other than the telemetering requirements which are required for the engineering aspects of the system.

To obtain the one second of arc guiding accuracy is somewhat of a problem right now. We are not at all sure whether our experiment has in it the capability of giving us an error signal which will allow a guidance system, in the satellite, to point to one second of arc. There is a possibility of using the zero order image in the spectrum. We are looking into it. We do not have a solution at the present moment.

Now, exposure times, let us say on an individual star, for one angstrom resolution, the minimum exposure time will probably be of the order of five minutes. The maximum exposure time is probably of the order of 200 minutes for a star with one angstrom resolution. If we go down to our bandwidth of 50 angstroms resolution, the exposure times here are about the same. This is of the order of five minutes again, and this time up here has been cut down maybe a factor of ten, something like that, of the order of 20 minutes. So it will take between five minutes and 20 minutes for us to look at a particular object and get all the data back.

This presents a problem because it looks like an awful lot of objects can be gotten in this five minute period of time. So this means if you work only the five minutes of time to get the kind of statistics that we deem necessary, there will be a large part of the orbit over which our instruments may not be gathering data for us. Unless the instrument has the facility of being able to be programmed at least to a small amount in advance from the ground, that is, after a certain object is finished, we might be able to program the telescope to start finding some other object in the sky, something of this sort. There is one additional engineering thing we are going to need in the telescope. That is, we are going to have to have some way of disabling or keeping the system from looking at the sun. Now, this could be done, let us say, in a couple of different ways. One, we could put a shutter across the primary mirror such that when it got too close to the sun it would close. This is one possibility. Another possibility might be that one would have a photo cell system of some sort, which, if the light was above a certain amount, it would disable the system and move it away from the sun. How this is going to work at the present moment is mainly an engineering determination of what sort of system fits best into the overall capabilities of the system.

I think I had better stop right now. I shall be glad to answer any questions.

DR. ROMAN: Thank you. Before you do, I think there are people standing. I believe there are two or three seats over on this side of the auditorium if you would like to try to find them.

I would also request that for the sake of the reporter before you ask a question, you state your name and company.

Finally, one other announcement which I should have made earlier, and that is that there may be representatives of the press here.

Now, are there any questions on the Goddard experiment, the stellar spectrophotometer?

DR. HELVEY (Radiation Inc.): Just one question. You mentioned that data storage system will be furnished or will be manufactured in-house, at your place. In other words it would not be farmed out to industry. Is that right?

MR. MILLIGAN: No. Since we consider this part of the experiment essentially, we feel that since it is so crucial to our experiment that we will consider it part of our experimental apparatus and that the weight allocation probably and the authority or the blame will be placed upon the experimenter to have a workable system. I am not saying that we are going to build the system in-house. Obviously we don't have the capability.

DR. HELVEY: Yes, because we have special capabilities in this field. That is why I asked the question.

DR. ROMAN: I think perhaps the main distinction as to whether these things are the responsibility of the NASA directly or of the individual experimenters is that the contracts from the NASA part of the program will be let directly from headquarters here, whereas as far as the aspects which are involved with the individual experimenters, they will be handled by the experimenters.

DR. HELVEY: Thank you.

MR. HALLOCK (Gruman Aircraft Corporation): Can you elaborate on the grating distance?

MR. MILLIGAN: The grating will probably be a plain grating. It will probably be a 15,000 line grating of the order of 10 inches across.

MR. MITCHELL (Boeing Airplane Company): In talking about the guidance, the point source, you mentioned one second of arc, and then called it "drift". Is it absolute pointing accuracy or is it a rate which is more critical?

MR. MILLIGAN: The thing we want to be able to do is to hold the telescope pointing at a star within one second of arc for a period of, let us say, 40 seconds. Now, it can drift plus or minus that, plus or minus a half second of arc during those 40 seconds.

MR. MITCHELL: What is the absolute pointing accuracy you need to start with? The field of view I think was one degree.

MR.MILLIGAN: The field of view on this will be of the order in arc, at least as far as our experiment goes, of the order of three seconds of arc.

DR. ROMAN: This means that the pointing will have to be within a few seconds of arc.

MR. MILLIGAN: And after we lock onto it, we will have to hold that accuracy plus or minus one half second of arc.

MR. KIRCHOCK: (Pierpont): Have you considered the use of a light funnel to reduce the accurate pointing requirement of the spectrometer?

MR. MILLIGAN: No, we have not. Actually, this project has only been under way a short period of time. We have a lot of design work left to do on our experiment, an awful lot of it.

DR. ROMAN: Am I right in thinking that a light funnel would not solve the problem because the problem is to exclude other regions of the sky? It is not a matter of funneling the light into a small region of the instrument.

Any other questions on this experiment?

MR. TRIPLETT: You mentioned one second of arc guidance for 40 seconds. Later on you spoke of exposure time of five minutes.

MR. MILLIGAN: Each sample will be taken on the order of 40 seconds. Then we will have to move the grating. It will have to come back on again and hold it for a given period of time.

QUESTION: The problem of the light funnel is not so much the extended region of the sky, but to maintain the collimation. You have to fill the collimator and nothing else.

DR. ROMAN: Any other questions or comments from the audience on this?

In that case we will proceed to item 3 on our agenda, the Ultraviolet Photometer, the University of Wisconsin, and I will ask Dr. Code to discuss this instrument.

ULTRAVIOLET PHOTOMETER, UNIVERSITY OF WISCONSIN, BY DR. A. D. CODE.

DR. CODE: Let me say something first about the astrophysical problems that we are concerned with. I think you will see that there is similarity in the problems that we are dealing with, and those of all the stellar experiments, that which was described by Dr. Kuperian and the Princeton program, in that we are interested in measuring the radiation from stellar objects and from gaseous nebula or interstellar gas in a restricted wavelength region.

At the University of Wisconsin for a number of years we have been concerned with two problems in which the extension to the ultraviolet is just a logical extension. One is the energy distribution of stars. The second is intensities of emission lines in gaseous nebula. What we really want to know are, let us say, the number of ergs per second per square centimeter per angstrom incident on the outside of the earth's atmosphere from a stellar source.

(Blackboard demonstration.)

DR. CODE: For example, if we plot the energy as a function of wavelength for a star similar to the sun, say, we can determine such an energy curve from one or two microns to .3 of a micron, and this pretty well includes all the energy distribution of the star. We have good leverage when comparing the energy distribution of the star with theory.

Let me extend this axis on down to the ultraviolet. For the hotter stars, say a star that is equally bright in the photographic region, we expect the energy distribution to look something like this. Most of the energy will be in the ultraviolet. In other words, we make observations of this part of the spectrum. We are not learning much of the stellar energy distribution. Therefore we are not able to fit theoretical computations for stellar atmospheres to the observations with any great certainty. At the present time we observe energy distributions with a band width of the order of 10 angstroms by a scanning spectrograph similar to that which Dr. Kuperian described, a rotating grating. We scan across the spectrum and derive such energy curves. An important feature, however, is that we have to have the instrument calibrated. We want to know what the sensitivity function of the whole instrument is, at any rate. It is not so important to know exactly how many ergs per square centimeter per second. We want to know that there is twice as much energy here as here. For terrestial observations, this is simple in a way. It is an extremely difficult proposition, but at least we can observe some standard radiation source, and then observe the stars.

For observations in the ultraviolet from a space vehicle, we are going to have to calibrate the instrument and then hope that this calibration remains or build in some way of checking the calibration. Now, there is one built in way for checking the calibration, and that is to keep going back to the same star. Similarly with band widths of the order of ten angstroms one can measure the intensity of the emission lines and diffuse nebula, and from these lines possibly determine the temperatures of the nebula, density, and possibly something about the space distribution of the gasses, too, the non-uniformity density of the distribution. But what we are not able to do in this case is separate very well the effects of different chemical compositions from effects of temperature and densities. There is some of the order of 100 emission lines that you would expect from the region of 1,000 angstroms to 3,000 angstroms that would give you a great deal of leverage on this problem. So we would like to carry out the same kind of observations we have been making into the region from 1,000 angstroms to 3,000 angstroms. Initially, we thought that a considerable amount be learned from wider band widths, a multicolor photometry. By wider band widths I mean delta lambda of the order of 100 angstroms, and that we could probably isolate these bandwidths with combinations of the spectral response, of detectors and filters. In such a photometer, then, to sketch it roughly we have managed perhaps getting four simultaneous measurements in four different wavelengths from the ultraviolet with four sepaate off-axis paraboloids.

We are looking down at this device. Here are the four mirrors and the focus for each one is brought off to the side here through diaphragms that we can change and filters that we can change to a small mirror with a field lens and project an image of the telescope objective on to a photomultiplier. Then this system would include both the acquisition of the star or the finding of the object and the measurements. The procedure would work something like this. When we are hunting for the star, we would incorporate a set of solar aspect cells that would tell us the angle between the optical axis of the telescope and the direction of the sun to, let us say, one degree. Then we know that the telescope is pointing somewhere in this one degree cone, scan around with two of these photomultipliers operating, with one degree diaphragms, one of them a yellow filter and one of them a blue filter. So if we get a deflection, this is a star somewhere in this one degree cone. We have measured the brightness in the yellow and the color, this yellow-blue base line, this is sufficient to tell us the name of the star. Then we would put in a diaphragm of the order of five minutes of arc, and attempt to center the star in this diaphragm and then we are ready to make the observations in which case all four cells are operating at four different ultraviolet bandwidths with 10 minute of arc diaphragms.

Now, if we wanted to observe early type hot stars with 10 inch mirrors and, let us say, something like 10 per cent efficiency for the optics and a 20 per cent quantum efficiency, as some kind of guess, with a 100 angstrom bandwidth and a star that is sixth magnitude photographic, we would be dealing with something of the order of 3,000 photoelectrons per second or a signal to noise ratio of the order of 50 to 60 for a measurement in one second. This of course is a guess. The purpose of the experiment is to really find out how good the guess of the number of events is. Therefore, since we really don't know how bright these objects would be, and we would like to work over a range of densities, we would like to have a dynamic range of the order of 10 to the sixth, and we would like to achieve a one per cent accuracy in the measurement.

Now, we have imagined perhaps doing it in this manner. First of all, while you are searching you would have to have continuous transmission of data so that as you pass over the star you know right then that you have the object. So that we have imagined some system of this sort.

Let us say here is one of the photometers. We would use this photometer to moderate an audiooscillator and actually two for each of the photometers; one that would give the gain of the amplifier involved in this photometer and another the counts. For example, suppose we present this in an analogue way. Here is a deflection from the star. If this deflection exceeds 80 per cent, then we change it again by a factor of ten. If the deflection drops back down to the order of 30 per cent, then the gain changes. That is, the gain is going to be constant over this range from 30 to 80 per cent. So we would give the gain of the amplifier and the deflection with that gain. Then this would be sent out with an FM transmission. When we are actually making the observations, with the oscillator, pulse shaker, we count the pulse and store, there are two binary counters for the gain, and the deflections, we can store these, perhaps with a tape recorder, or transmit the pulses directly.

It is some system like this that we imagined for the simple photometer in which case the band as far as the servo loop in finding the star, it means that the presentation of the data at the ground station has to be quite fast and efficient. We want to be able to make a decision right now in order to center the star in this diaphragm.

Now, the next logical extension is to narrow the bandwidths and a larger choice of bandwidths, which leads us to a spectrograph similar to that described by Dr. Kuperian. The one feature that I might point out is this. From the standpoint of, let us say, guidance for a given astronomical problem, spectrophotometric problem, a pertinent quantity is the ratio of the angular patch of the sky which you can take in with your entrance diaphragm or slit to the spectral region you can isolate, ratio of angular resolution to spectral resolution. We would like this number to be as large as possible. If for a given spectral purity we could take a very large patch of the sky, then as long as the sky background is not causing difficulty, it means that your pointing accuracy is not as high, the requirements for pointing are not as high providing you meet this ratio. This is just a function of the angular dispersion of the grating, the angular dispersion of the grating and the ratio of the diameter of the collimator to the diameter of the telescope. The bigger the bundle of light you take, that is the bigger the grating and collimator, the less accurately you have to point the telescope. The higher the angular disperson -- let us see, this is radians per angstrom, the higher the angular dispersion, the less accurately you have to point your telescope.

QUESTION: Do these backgrounds diffuse intensity or are they all point sources?

DR. CODE: These considerations would apply to both point source and nebula. Let us think of it for a moment -- we are pointing at a star, how wide can we make the slit, how wide angularly can we make the slit? Now we have to hold the star within this slit width.

DR. ROMAN: Could we have your name and affiliation?

MR. WHITNEY: My name is Whitney with Thompson, Ramo and Woolridge.

DR. CODE: The reason I mention this is that you are going to hear about requirements for angular resolution greater than one angstrom, a tenth of an angstrom or so. Thus, if we are going to have a bandwidth of ten angstroms, the pointing accuracy required is one hundred times less than it would be for any system requiring a tenth of an angstrom resolution.

This kind of spectrophotometry does not put as high a demand on pointing or also upon the length of time that one wants to store. It puts the same requirement onthe amount of data that one is handling as the higher resolution experiments you will hear about. So if you provide a stable platform that will take care of that, then this problem is adequately taken care of.

DR. ROMAN: I invite discussions of the Wisconsin experiment.

MR. FRIDGE: (United Aircraft Co.) I did not understand why you were using four mirrors which have to be aligned carefully and maintained that way for a long time. Why not a single mirror?

DR. CODE: Because in this case they do not have to be maintained to get them to focus in one place. Five minutes of arc is not a very serious requirement.

MR. FRIDGE: Will that maintain the spectral accuracy?

DR. CODE: The band width is being maintained by the transmission of filters and spectral response of the receiver. We are just trying to collect light in a pretty big hole. So it does not have severe requirements on the collimation of the optics. This way you introduce the least number of deflections.

MR. FRIDGE: Are the paths well separated?

DR. CODE: I don't know precisely how to isolate the spectral regions that I am interested in as yet. There are ways of doing it now. There may be better ways six months from now. They would overlap if we were to do it right now.

DR. ROMAN: Your aim is not to have them overlap, but you are not sure you can do that, is that correct?

DR. CODE: That is right.

MR. HARNED (Lockheed Missile & Space Division, Palo Alto, California): Do I understand you are identifying the stars by making a photographic measurement in terms of your color index and determining what star you are looking at from this?

DR. CODE: Plus the fact I know the angle between the sun and the star to one degree.

MR. HARNED: You have an initial one degree area. You can determint from this exactly the point you are on?

DR. CODE: Pardon?

MR. HARNED: You can determine exactly what star you are on, after you have separated out the one degree region?

DR. CODE: I know the star is somewhere in this band one degree wide. Then I know the brightness of the star and its color. If the star is bright enough this is quite unambiguous.

DR. ROMAN: Any further questions?

DR. KUPERIAN: Could I ask you what dynamic range your telemetering data has?

DR. CODE: Well, ten to the sixth is what we wanted to cover. But we have counters that get us, well, sufficient data for ten per cent accuracy actually. I talked about two channels, one for the gain of the amplifiers and one for the counts. These were a 10 stage binary counter and a four stage binary counter, a four stage binary counter that records the gain, and a 10 stage binary counter for the deflections. That is more than adequate for dynamic range of ten to the sixth and one per cent accuracy.

DR. KUPERIAN: That is one per cent everywhere for ten to the sixth accuracy?

DR. CODE: That is right.

DR. KUPERIAN: This data is simultaneous on four channels?

DR.CODE: That is right.

DR. KUPERIAN: And it could be stored?

DR. CODE: It can be stored. We had imagined that this system adds considerable redundancy, too, Perhaps one of these channels fails. You have three of them or two of them working. Perhaps the storage system fails. You can still get the data pulses directly, or, let us imagine that the guidance goes haywire, and you really don't know where you are pointing; you can put this on this continuous data transmission that you use to find the star. Then you just sweep the sky and you have to untangle the results you get but you can get some results.

DR. KUPERIAN: One more question in this connection. I am trying to bring out some point that it tends to generalize the system rather than be specific. Your pulse counting here, in other words, you have an event you wish to totalize in some instance.

DR. CODE: That is right. We want to code in some way. We don't want to send down every pluse.

DR. KUPERIAN: In other words, it is digital data or pulse data.

DR. CODE: You start off with a DC amplifier, a current device, and then convert it to digital in the scheme that we have imagined, and digital perhaps as much for getting through the noise and getting the signal down.

DR. KUPERIAN: You have some sort of current integrator that puts out a pulse after you have integrated so much. You speak of DC amplifying equipment.

DR. CODE: There is a blocking oscillator whose pulse rate is determined by a voltage output from this 100 per cent feedback DC amplifier.

DR. KUPERIAN: So it is not a tight current integration.

DR. CODE: That is right.

DR. FOSTER (ARC): I have a couple of questions. You intend using the same detector for the UV as for the red, rather blue and yellow identification?

DR. CODE: Two of these photomultipliers would be the photomultipliers in a lithium fluoride bottle, and they would not go as far to the ultraviolet as the other two. Two detectors are capable of observing in the photographic and visual region. They are not capable of going as far to the ultraviolet. So these two channels are things like 2500 angstroms for the ultraviolet and 1800 angstroms, something of that sort.

DR. FOSTER: I have one other question. Then you apparently intend scanning in the sense of this range one degree wide, and as that slowly goes across you will look at the yellow and blue intensities to decide whether you want to stop at that one or not?

DR. CODE: That is right. Actually, one would have two speeds, a slewing and a setting. You go on past, you get a deflection, you come back with a setting and try to get the star in a hole.

DR. ROMAN: How slowly do you think your slew speed would have to be in order that you get the deflection?

DR. CODE: This depends on how faint a star you are going to observe. It would probably -- well, about a half a radian per minute would allow you to locate the second magnitude stars.

MR. BOLLING (Chance Vought Inc.) Dr. Milligan mentioned that storage would be his responsibility in the experiment. Dr. Code, have you thought of the same responsibility on storage?

DR. CODE: It is certainly an integral part of the experiment. This would be part of the package that includes the optics.

MR. BOLLING: On the data transmission, have you considered the "bits" per second, or the band width for your real time transmission?

DR. CODE: Occasionally I have come up with such numbers. This does not seem to be a very critical thing from our standpoint, namely, how many "bits" can you handle. This is just determined by how long we have to work on one star.

DR. ROMAN: Coming back to this question of on board storage being the responsibility of the experimenter, I think this is something we are going to have to work out. Almost all of the experiments will require on board storage. I think that we will probably try to develop a single system that everyone can use in which case it would no longer be the sole responsibility of the experimenter. On the other hand, the experimenter is certainly going to have to have appreciable input into what this system will be. I think from the standpoint of the contractors this is not an important question at this moment, since it probably will be contracted out regardless of who does the contracting.

Are there other questions?

MR. COOK (Space Technology Labs): I would like to know why the figure of 1,000 angstroms was decided on as the lower wavelength limit, that is, aren't there hot stars that have an energy below 1,000 angstroms which would be of interest? Also, would there not be spectral lines in this region from stars of interest which could be observed with a spectrograph?

DR. CODE : Actually this point here was supposed to be about 900 angstroms. At the Lyman limit 912 we expect the energy distribution of stars to just drop off completely. We expect the interstellar medium to be quite opaque. So we don't expect to find too much. But just because of this prediction, we ought to try it. That is why one of the bandwidths is there. We really would be surprised to find a great deal of radiation beyond the Lyman limit 912.

DR. ROMAN: Do you have anything to add?

MR. MILLIGAN: There is also the additional problem, as some of you may know the reflectivity problem in the ultraviolet is quite serious. At the present moment the present state of the art is such that if we are going in the region below 1100 angstroms, your reflection falls off a very good mirror drops off to about 10 per cent.. In fact, you are very lucky if you can get 10 per cent. This coupled with the fact that if you want to fly a window detector, and lithium fluoride looks like the best one you can use, it has a crystal cutoff of 1050 angstroms, so the combination of the reflectivity, the interstellar absorption, things of that sort, and the detector problem tend to limit us a little bit.

DR. ROMAN: The next question?

MR. ANSELM (Beech Aircraft): In regard to the portion of the experiment on nebular gas mass are you interested in verification of gas constants? My reference would be in regard to the determination of speed of sound at very, very high altitude.

DR. CODE: I don't quite understand. In the gasseous nebula you are referring to?

MR. ANSELM: Yes.

DR. CODE: This of course is a feature -- I don't know of an experiment -- we think we know in the gas nebula what the speed of sound is. We can measure it by the number of electrons.

MR. ANSELM: Do you have verification of it?

UNIDENTIFIED: We are doing some work at the present time on the accoustics.

DR. CODE: It is possible to tell whether the excitation of spectral lines is due to radiation or collisional excitation. This it would be intended to explore.

DR. ROMAN: The next question.

MR. McCLOSKEY (Aerojet General Corporation): Will a copy of this be made available so that we don't have to keep notes?

DR. ROMAN: I think it would be safer for you to keep notes on the areas in which you are interested. I doubt that we will be able to make the entire proceedings available to everyone.

Are there any other questions? If not, we will pass on to the Ultraviolet Sky Mapping experiment, by the Smithsonian Astrophysical Observatory. Mr. Davis.

ULTRAVIOLET SKY MAPPING, SMITHSONIAN ASTROPHYSICAL OBSERVATORY, BY B. DAVIS.

MR. DAVIS: First, the reason why there were so few of the preliminary specification sheets brought down here was that it still was a rough draft, and we did not want too many obsolete copies drifting around three or four weeks from now when we get that whipped into shape. Anybody that does want the smooth version of this rough draft and possibly extra copies of the drawings that went with it should write to us. I think the address is on here.

DR. ROMAN: Perhaps you had better read it, since the ones who do not have the copy will not have the answer.

MR. DAVIS: Smithsonian Astrophysical Observatory, Cambridge 38, Massachusetts. We will send out copies when they are ready.

The information we are interested in is the same as Dr. Code and Dr. Milligan are interested in, the spectral intensity distribution of stars and interstellar matter.

There are approximately 10 million stars brighter than magnitude 15 photographic.

DR. ROMAN: For the sake of the non-astronomers we might add that this is roughly 10,000 times fainter than can be seen with a naked eye, a little less.

MR. DAVIS: Yes. It is roughly the limiting magnitude of a 20 inch telescope with good photographic film in a few seconds exposure time.

These stars have a temperature distribution -- number of stars versus temperature with 50,000 degrees up here. The sun being about 5,000 degrees, the lower end we don't know too much about yet. But the numbers are roughly like that. The hotter stars, there are very few, probably it falls off even more rapidly than this.

(Blackboard demonstration)

MR. DAVIS: Of course, when you get down to zero temperatures, there are zero stars, but it is fairly flat in the area from about 10,000 degrees to 3,000 degrees, roughly equal number of stars at all temperatures. Then these really hot objects where most of the ultraviolet comes out, where most of the light comes in the ultraviolet, there are very limited numbers. When we start pointing telescopes at the sky, that are sensitive to ultraviolet, we start picking up mainly the hot stars and not so much the cooler ones. For instance, we may have a telescope with a two degree field of view sensitivity in the ultraviolet, and out of the ten or so stars that may appear in that field that are sensitive enough to be picked up down to the 15th magnitude photographically, the hotter stars, about nine out of ten of these will be down in this distribution tail of hot stars. Only one of ten or so will be as cool as Sirius, 10,000 degrees, and practically none will turn out to be as cool as the sun if present theories of astrophysics are correct.

What we want to do is take a look at every star in the sky bright enough to show up in our system, see what it does look like, or at least refer to the fact that it is there, In order to do this in a reasonable amount of time we can't use diaphragms and photocells. We have to use either television or photography. For many experimental reasons we have decided that television is the answer to our problem. Taking photographs, or pictures, rather, of fields of the sky about two degrees square, there are roughly 40,000 square degrees in the skies, so this means 10,000 pictures in which color our equipment is sensitive. In this way, whether astrophysicists think stars are interesting or not, at least we will have it and we will know whether it is interesting. About 90 per cent of our stars will be hotter than 10,000 degrees. Hopefully, if everything goes right there will be a few of the brighter stars coming through that are down in the cooler temperatures where most of the radiation does come out, where we can see it, but interesting things may happen in the ultraviolet.

Taking this special distribution curve again, for the sun the curve is something like that. For a hotter star such as Sirius, the curve will be more like that. We can observe down to about 3,000 angstroms. These curves are not quite drawn in right. In the case of the sun, we have continued the curve on down below 3,000 to the extremely short wave lengths. In the case of the bright stars, there is at present no observational information below this point.

Now, by these means we can trace this curve by putting three points on it, or with spectroscopic resolution we can obtain the full curve. Television techniques will put on the three points at 2600 angstroms, 1900 and 1350. In addition, since television does not give really accurate magnitudes, we are hoping for ten per cent over a dynamic range, possibly split up into separate ranges from the nature of the beast, a total dynamic range of 10 to the fourth, accurate to 10 per cent. If we were to add a photoelectric device we could get down to one per cent accuracy, and we do see putting an extra telescope into the platform with five minutes of arc diaphragm to isolate one star near the center of the field and look at it more carefully in six rather than three spectral bands. Each of these three bands will be taken care of by its own special telescope designed for sensitivity within a few hundred angstroms of the particular band. So together with the photoelectric device that adds up to four telescopes all of them identical, with reflectors eight inches in diameter,20 inch focal length.

We also foresee adding a fifth telescope with an objective prism, no slit involved in this device. It merely takes the same field and spreads the brighter of these stars into spectra. This one will probably be the brightest star in this field. It will be pre-selected for that. So that maybe three stars out of -- well, actually it would be close to three stars out of a hundred-- we would get slight spectra for it, and thereby cover the whole sky, getting, with the sensitivites we perceive, possibly as many as a million stars in the multicolor pictures and about 20,000 stars with slight spectra. The exposures required to obtain these sensitivities are ten seconds long. If everything goes right, exposures of one second will be possible, but we think we need ten seconds exposure to obtain adequate sensitivity. In order to separate close double stars, we want one minute of arc resolution. The television image tube is already been developed by Westinghouse Research Laboratory in Pittsburgh, and we have developed the optical system, it is already under construction. So the first two components of the block diaphragm are pretty much under control.

The aberrations and the image tube are matched to give one minute of arc resolving power. The remaining requirement is that the instrument be stable enough for ten seconds of time so that these images do not blur and that we know where we are pointing. We think both of these requirements are fairly well met. Then if this diaphragm is to be centered around a star, we have an additional requirement that we be able to determine by a pre-program device either from the ground or some other way, within five minutes of arc pointing accuracy. If we have that, it is very easy to find out exactly where it was pointing by looking at these pictures afterwards.

One possible method is a finder telescope with television to send back about a 15 degree field of the sky with sensitivity the same as we have on the earth and compare it with star maps. If we know within one degree where this is by astrophysical theory, we can identify these stars and measure it to one minute of arc.

Then the next step in our system is the TV camera. We are fairly certain now on what we need for a TV camera. We have done no design work. Some preliminary feelers have gone out, that is all so far, on a television camera to drive the image tube. But looking at the amount of information, each square minute of arc in a two degree field has ten possible bits of information for the brightness of the object in that particular space. Since we do not know that we are looking at stars we cannot use pre-recording, because we are also interested in what the nebulosities are like. . Of course, with the slit spectra the object is spread out into lines. Or if they are small nebulae we might even get a stream of points representing its emission lines. There are 144,000 more or less in this size field, 144,000 -- this thing comes out different this time than the last time I figured it out -- there are something like 250,000 bits of information contained im one of these fields, so that storage in the satellite is something we have not contemplated. We plan to use live television throughout, controlling it from the ground when it is within view of the station and just letting it sit there for the other 90 per cent of the time at as low a power drain as is feasible. In other words, merely have some kind of radio receiver on so that we can command the transmitter the next time around.

Using this method it would take about four months to cover the entire sky. The amount of information involved is very high. We need a scanning rate of one frame per second. We need a 150 kilocycle band path merely to transmit the picture to the ground. We will need to store this information on the ground in a method that is amenable to automatic computing so that the astronomer does not have to do the whole thing with pencil and paper. But in the satellite we will/not store it. From here it goes into the telemetry which is the responsibility of NASA and the working group, until it gets back down to the ground storage again where it comes back to us.

The mechanical end of this to hold the optics together rigidly has not been worked out in detail. The drawings show how far it is. We know the dimensions. We do not know what type of hardware we will be using to hold it together. All the moving parts that we think we will need are shutters to close off from the sun if it comes too close. Everything else will be done by electron standards.

We also have in mind some spectroscopic slits spectroscopy similar to what some of the other institutions have mentioned, but have not gone to the design of additional instruments up to this time.

I shall try to answer any questions you have.

MR. DEL DUCA (Ramo-Wooldridge, Inc.): This looks like a lot of power. Have you any idea of the order of magnitude of the power? Two, and this applies to all the rest of the presentations, is the experimenter going to provide any secondary source of power in addition to the primary vehicle power? Third, what kind of auxiliary or secondary power are you thinking about if you are thinking about it.

MR. DAVIS: In the first place, most of our power requirements are motion of the platform, the heaters for the television tubes, and the transmitter power for the telemetry. These are roughly equal and probably in the vicinity of 10 watts each for 20 to 60 minutes per day. I do not think this requires additional power beyond that that is anticipated for the satellite.

DR. KUPERIAN: How much is that part without guidance?

MR. DAVIS: Taking off this?

DR. KUPERIAN: Yes, the platform part.

MR. DAVIS: Just these three areas, almost all the power goes into the television heater which totals something like ten watts.

DR. ROMAN: Does anyone else envisage power requirements appreciably in excess of these?

MR. DAVIS: The transmitter power depends very greatly on what we have at the ground receiver. We were figuring this on 60 foot dish antennas. If the antenna on the ground is smaller, we need more power upstairs.

MR. DEL DUCA (Ramo-Wooldridge, Inc.): In case the vehicle power, main power supply, goes out so that you don't have anything for attitude control or guidance, you would like to just get what you can out of the experiment, do you have a secondary source contemplated?

MR. DAVIS: We would like to put a nuclear power source in. We are not sure we will be allowed to.

DR. ROMAN: Are you sure your cathodes would like it if you were?

MR. DAVIS: Yes.

MR. CHENG (Hughes Aircraft Company): Mr. Davis, your slewing time between frames, I recall faintly the last time I met you, would be something of the order of 10 seconds.

MR. DAVIS: Yes.

MR. CHENG: Does that still hold?

MR. DAVIS: That is what we are aiming for.

MR. CHENG: Could you possibly allow a longer time maybe lengthening the experiment somewhat?

MR. DAVIS: If you double that time you multiply the total four months by one and a half and end up with six months, we would still be happy.

MR. JOHN LINDSEY: I was wondering if you have looked into the possibility of storage using two systems -- frankly I don't know whether the resolution is adequate or not -- but one a Vidicon where you store it temporarily on the photoconductor, and second, a static electricity type recall which is being developed for TV work.

MR. DAVIS: We have been talking with RCA about this. They say it looks hopeful. This would reduce the four months that we need. It would cause a band width difficulty getting the information out in time when it is over the station. We would be happy to have it but it is not necessary for the experiment.

DR. ROMAN: Along the same lines have you considered photographic storage? Obviously you can't use photographic film with the idea of recovery, but what about storage of photographic film and scanning?

MR. DAVIS: We have considered this. If such a device could be made "fail-safe" we would enjoy very much having it. By fail-safe, I mean when you run out of film you can still use the machine, or if the film jams, you can still use the machine. If we can get these two points we would like very much to have it. Again it is not necessary, but it would help the experiment.

MR. LINDSEY: I have one thing which actually does not have anything to do with this experiment. The Space Technology Laboratory has asked why cut off at 900 angstroms. I am wondering if the answer to this is not really expediency? We know in the case of the sun that Rense, Colorado, Tousey and Hinterreger, AFCRC, have been working with the hydrogen 304 line. I think the people here might like to know if they could do it. I really don't know what the energy transmission might be, but I think the people in the future might be interested in looking at the short wave length, is that not true?

MR. DAVIS: In regard to the short wave lines from the sun they put an amazing amount of energy on the earth from the sun, but you take the next nearest star, Alpha Centauri, there are roughly four lines that we could pick up with this type of sensitivity. You take stars that we know nothing about, and we can't even make guesses. The theory of cool stars in the ultraviolet is pretty close to zero so that we can't make really educated guesses about how these stars will look. Yes, we want to look at them as far down as we can get.

DR. ROMAN: Dr. Savedoff, have you gone far enough in your work, in the far ultraviolet, to have comments on this question?

DR. SAVEDOFF (University of Rochester): We have not gotten very much further than we have been. We have been very much encouraged in using ordinary techniques, spectrographs and filters. The 304 line is still very much shaded by the interstellar hydrogen. It looks like the universe opens up again somewhere at a hundred angstroms and lower. We are very discouraged. When you think of working at one angstrom, you can use the platinum reflector. You have to be within one degree of the plane of the mirror in order to get reflection. That is a horrible requirement.

DR. DAVIS: We are looking into the use of zone plates, also. They start getting very difficult to make when you get below 50 angstroms, but we have hopes to use them. We have hopes of having zone plates in the near future that are good down to possibly 50 angstroms for focusing that way.

DR. LILLER (University of Michigan): I might just say that Professor Lawrence Aller at the University of Michigan has calculated some of the effects of the interstellar medium, and I think he is in agreement with Dr. Savedoff with one hydrogen atom in a cubic centimeter you get quite a number -- from 912 angstroms, and the Lyman wave limit to shorter wave length down to 100 angstroms range, and below, everything is pretty well blocked out. Below 100 angstroms you will start to see something. But between 100 and 1,000 angstroms, space should be pretty black.

MR. LINDSEY: That sounds like the answer still is that we are interested.

DR. ROMAN: But it is difficult to do.

MR. STEIN (Republic Aviation Corporation): Are you definitely ruling out the idea of recovery of information by the capsule technique?

DR. ROMAN: My feeling is that it is going to be extremely difficult to oonsider recovery of anything from such a vehicle without (a) changing its orbit, and therefore destroying its future usefulness, and (b) changing the balance of the instrument, so that even if itt could be used it would be extremely difficult to rebalance it to within a part of ten to the third or a part of ten to the fourth.

Any other questions? I think then we will go to the last of the stellar experiments, and then we will have a recess after that. I will call on Dr. Rogerson of Princeton University to discuss a high dispersion stellar spectrograph.

HIGH DISPERSION STELLAR SPECTROGRAPH,
PRINCETON UNIVERSITY, BY DR. J. ROGERSON

DR. ROGERSON: Princeton University Observatory is interested in investigating primarily the dark gasseous matter that exists between stars. The best way that we now know how to do it is roughly the following.

Fortunately we have some very bright light sources scattered around space.

(Blackboard demonstration)

DR. ROGERSON: If this now represents a cloud, an accumulation of dark gasseous matter, which being dark we can not see of itself, we can detect its presence by investigating the light of that star after it has passed through the gas cloud. So this now represents our satellite. We will look primarily at very bright, very hot stars, and investigate the effect of these interstellar gas clouds on that light.

It so happens that the gas cloud has considerable effect except that the effect is restricted to quite narrow wavelength regions. If I draw you once more a bit of a star spectrum in the ultraviolet, this is lambda, and this is energy. Hopefully we might have a very hot star that has no lines of its own. Let me take that for the moment. We have some distribution of energy. What the cloud will do then is subtract a rather narrow line, in general much narrower than any lines that the star itself would have in its own atmosphere.

Now, I bring this point up because it means that we must be able to resolve the spectrum well enough to see this. Hence our experiment is concerned with high dispersion spectroscopy. So our experiment then is to look at these very bright stars, disperse the lights in a spectrograph, and in some manner to detect the presence or absence, as the case may be, of lines which we have pre-selected. That is from atomic structure, molecular structure, we predict at what point of the spectrum these lines should exist. So we do not have to scan the entire spectrum.

These lines, if we can say something about them in angstroms to give you an idea to compare them with the previous presentation, the width of these lines is perhaps of the order of a tenth of an angstrom in width, which is I think something like a full factor of ten over the narrowest bandwidth that we had before mentioned. That means that in order to detect this absorption in the starlight, we must be able to resolve roughly the same band path, that is a tenth of an angstrom.

So much for what we are trying to do. Let me show you a little bit of what our present thoughts are on how we would go about doing it, what the equipment is.

We have a little bit of a problem in that we need very bright sources and, as was brought out before, these bright sources are rather scarce. When I say bright sources I mean intrinsically bright sources, that is if you were up close to them, you could see they were bright. They might be very distant. By bright sources, I am talking of the fact they are bright in their ultraviolet. They have a larger part of their total energy in the ultraviolet than in the visible. For example, because of this requirement, there are not too many of those stars, and in order to have a useful instrument, we have to be able to use these stars even though they are quite far away from us, and apparently rather faint.

The upshot of all this is that we need a rather large apperture, not the largest presented today, but the second largest; 24 inches is our apperture. This is a 24 inch diameter light collector apperture. The "F" ratio of this mirror is, according to a light design, F/3. There will be a secondary mirror, Cassegrain secondary, which will change the overall "F" ratio to F/20. (See figure)

The light then is brought to a focus at, let us say, this point here on a slit with a spectrograph. The entrance slit of the spectrograph is of the order of five microns, very narrow. You can already see what sort of restriction this is going to make on pointing accuracy if we are not going to have this wander on and off the slit.

Located down here roughly in the center of the grating will be a concave grating. The whole spectrograph will be a Rowland type of spectrograph. The light will come down and hit the concave grating and form the spectrum along the edge of the Rowland circle. Along the edge behind exit slits of the same width we will have a series of photo tubes.

I might explain why we have so many. I have four here. There is actually a fifth one, but that is only for a minor procedure early in the orbiting of the satellite and I won't go into that at the moment. We have, instead of four separate tubes, that is for four separate wave lengths, we have two pairs. Two will be for what we call the far ultraviolet, which may go down as far as 800 angstroms and may be up to 1600 angstroms. The other will go and take over at 15 to 16 hundred angstroms and go to 3200 angstroms.

Why do we have two? It comes back to the problem, how well can this thing be guided, once it is up there? It is quite possible that perfect guiding is impossible. Ours will oscillate back and forth with some amplitude according to the sensitivity of the guiding and it will cause a fluctuation of the reflected light. That fluctuation we are not interested in. In fact, if we get that fluctuation, it will distort the information going to one tube. What we are now thinking of is having two tubes and only on the ground taking the ratio of the output of those two tubes so that as the light intensity is fluctuating as it comes to the angstrom slit of the spectrograph it will affect both tubes by the same factor. If you take the ratio that factor cancels out.

That system is not new. It has been used several times already. So that explains the two pairs of tubes.

Now, in each pair we will have one which is held at a fixed wave length, and the other will scan by that spectral line which I have drawn previously. So that now as we take the ratio we hope there will be a dip in the ratio corresponding to the dip in that spectral line as the slit scans by it and that dip will be a real dip which is in the spectrum, and not a dip because the star happens to be momentarily off the entrance slit. We plan all of this to be pulse counting equipment, the output of all of these tubes. It is not certain yet whether we will try to send down to the earth the accumulated amount of counts in a fixed time interval, or whether, as Dr. Milligan's experiment mentioned, we might count up to some standard number of counts, 10,000 or 100,000, a standard number, and then simply telemeter down the accumulated time that it took to gather or accumulate that number of counts. But the time I think would be digitized information also for the same problem, trying to get down the information as accurately as possible.

Now, this is our experiment. If I may take the liberty of going a little beyond our experiment into the satellite as a whole, because as we have been thinking of it, we have not been able to disassociate in our mind the requirements of the satellite from the requirements of our experiment.

DR. ROMAN: I was going to suggest that you do this rather briefly, because time is running on.

DR. ROGERSON: All right. Well, it is very important for us to know what star we are looking at. Even though in Dr. Code's experiment he can get an idea of what the star is from the color and the intensity with practically no ambiguity, we would like none at all. So we would like very much to have essentially a finder telescope, a telescope backed up by a television which would only be on as the satellite passed overhead, and would be able to give us a 10 by 10 degree field. We feel this is ample for recognition of constellation patterns so that we can pick our stars.

At the time that we find that, we hope by direct command to the satellite to be able to move the selected star into the field of our instrument.

Now, this is a rather large field to get this aimed properly so we hope then to have a second television system. There will be a mirror at this point in our design with a hole in it, the total field being of the order of a degree, perhaps a little less. This light would then come out and feed into another television, the fine scale television, and the field here would show a hole. What we would then attempt to do, as soon as we see our star come into this field, we would then only look at this television and force that star into the hole. Once it was in the hole, then it would fall on the slit, and on the guiding mechanism, that is the guiding center, which is at this point here. That is, you may have, for example, the slit just tilted so that light may be taken off to the side into photomultipliers and give you an error signal. So once it goes through the hole, it falls on the error sensing device, and it comes back to you people to do something about it, and then bring the image back.

MR. WHITNEY (Ramo-Wooldridge, Inc.): We have not been able to establish the angular pointing control you need without the diameter of the secondary mirror.

DR. ROGERSON: Let me say we want zero point one second of arc (0.1) for essentially an indefinite period. We expect that the satellite will not be available to us except once every 12 hours, for example. We were thinking of a polar orbit. During that 12 hour period we would be on a star and observe the entire time. We hope to have a command memory which will tell these photo tubes what wave length to go to and then set them in an automatic scanning operation. Once that scanning operation had been completed several times, according to the accuracy that we are interested in, then it would move on to the next line. So the telescope would be in operation all the time except during setting on a new star. That information would be stored and disgorged when the satellite came overhead. The storage capacity is not great for this type of work, being something of the order of 10 to the fifth bits in a 12 hour period.

Now I would like to go further, but I think I am running over. So perhaps we had better have questions.

DR. ROMAN: I would like to make one remark first on the polar orbit, that is, have you thought of the fact that you are going to have to go through that Van Allen belt in the polar orbit?

DR. ROGERSON: Well, it has come up. We have not decided how bad that would be for it.

DR. ROMAN: I have spent most of the time thinking we want to stay away from the polar orbit for that reason.

DR. TRAGESER (Massachusetts Institute of Technology): Does your equipment have any moving parts in scanning this while you are moving, while making this alignment?

DR. ROGERSON: Yes. Let me say this. During the scanning we scan slit width by slit width. It is not a continuous motion. The motion stops. We integrate for whatever time it takes to get 10,000counts, for example.

DR. TRAGESER: How long does it stop?

MR. ROGERSON: Of the order of, say, five minutes. After it has accumulated those counts, those counts will go into storage. Then the slit will move to the next band width and scan again. So this is not continuous motion during the integration, but step by step. The steps are equivalent to five microns being in the spectral range equivalent to about a 20th and a 10th of an angstrom.

MR. STAROS (Sperry Gyro Company): Dr. Rogerson, do I get the impression from what you say that more than one experiment is performed in a single satellite, yours with somebody else's, for example, or would one satellite be used?

DR. ROGERSON: I don't think that is for me to answer. There has been a fair amount of discussion along those lines.

DR. ROMAN: I think the way the program looks now the answer is yes, that there will be more than one experiment in the satellite. That goes for all the experiments which have been discussed. This is a point I was going to bring up later. It is just as well to get it on the record now. It does not look feasible, with the number of vehicles that we have available or expect to have available for this program, to assign a single experiment per vehicle and still provide any leeway for backup in case of failure.

MR. KERPECHAR (Kearfott Co., Clifton, N. J.): Assuming you are interested in a high pointing accuracy, what influence does the aberration of light from the star , due to the change of velocity in orbiting around the earth, what influence does that have on your experiment?

DR. ROGERSON: As far as the pointing is concerned, I hope the guiding will be sufficient to take that out. But there are doppler shifts which will amount to the order of one of our slit widths maximum. If when we store our counts we also store the time so that we know where we are in the orbit, we can take account of that fact.

MR. KERPECHAR: You can get five seconds displacement on a selected star if you are moving 25,000 feet per second. Is your accuracy of pointing one second for 45 minutes, something like that?

DR. ROGERSON: Twelve hours.

MR. KERPECHAR: What is your integration time? Due to displacement on the sphere of the star, how far would the star move in an integration period? What is the integration time?

DR. ROGERSON: The integration time is of the order of five minutes, I would say. I hope that the guiding is going to be active so that if the image does tend to move, there is a correction, we don't set it at one point and leave the satellite to its own devices.

DR. ROMAN: What you are reallying saying is that the fine guiding has to have sufficient capability to take out a little greater than a second of arc error due to aberration.

DR. ROGERSON: We are hoping that we can keep within that.

MR. TRIPLETT: The guidance system has to track a moving target. It means that its maximum rate of motion is something of the order of three seconds of an arc.

DR. KUPPERIAN: Are we talking about the parallax?

DR. ROMAN: No, aberration.

MR. MEYER (Martin Company - Space Flight Division): Can you tell me how long your five micron entrance slit is?

DR. ROGERSON: The accuracy in that coordinate is not nearly as severe as across the slit. It can be several millimeters long, I would say, instead of the five microns or so, at least in the one coordinate.

QUESTION: I have a question concerning experiment management.

DR. ROMAN: Could we leave that for a little later? I would like these discussions to relate directly to the experiment. There will be time for the other type of questions. Any more on the experiment?

MR. MITCHELL (Boeing Airplane Company): What is the resolution you can get from your secondary mirror for guiding purposes?

DR. ROGERSON: I am sorry, I don't understand.

MR. MITCHELL: You indicated from the experiment you would provide an error signal to the guidance. What sort of resolution can you give?

DR. ROGERSON: We are assuming that we can give you a suitable error signal corresponding to a tenth of a second of arc.

MR. MITCHELL: It has to be somewhat better than a tenth of a second if you are going to guide that.

DR. ROGERSON: What I meant is so you can guide within this accuracy. I don't know whether that is so. At Princeton University we have a balloon telescope project which will be going out there next year -- excuse me, 1961. That is designed to meet these specifics, ,to guide within a tenth of a second of arc. I am hoping we will get practical experience on just how that can be done satisfactorily. At present it is a tough job but it does not seem to be impossible to provide an error signal that will allow a good servo mechanism to keep this pointing within a tenth of a second of arc.

MR. CHAMBERLAIN (Hughes Aircraft Company): What is the size of your grating?

DR. ROGERSON: It will be something like two inches square.

DR. ROMAN: Any further questions?

MR. HYDE (American Optical Company): We are working with fiber optics configurations. We are able to take a bundle of these optical fibers which is round at one end and rearrange the fibers so that they are in a straight line at the other end. Now, the positioning of the star has yet to be within the area of the round bundle at the incident end, but as the star wanders on this round bundle it comes out of fibers which are up or down in different parts of the entrance slit. By this method one can trade a tight tolerance in one dimension and a loose one in the other dimension for an intermediate tolerance in the two dimensions. Now we don't have fibers that work down in the far ultraviolet. We can't do this at this time. But this might allow you to compromise from this very severe tracking requirement you have, because of your requirement for very high resolving power.

DR. ROGERSON: I would consider the fact that the bundles are probably not transparent rather serious.

MR. HYDE: All I am saying is that we have not made fibers out of the materials that you have to use in this kind of system. We don't have any different requirements than you do in other places for providing transparent materials.

DR. ROGERSON: That is entirely true.

DR. ROMAN: I think this is a good point to take a recess before we go on to the solar experiment. I suggest that we have about ten minutes to stretch our legs.

(Short recess.)

DR. ROMAN: I think now that we see how the time is running, it is obvious that we will have to have a short session this afternoon. I think the arrangement will be that we will now go to a discussion of the Michigan solar experiment. We will then have a discussion of the engineering aspects of the orbiting observatory and a few brief remarks on the engineering aspects of the experiments themselves. We will try to break for lunch at quarter to one. I suggest that we reconvene about two o'clock. Then we will devote the next period of discussion to the management and administrative aspects of the program. We will finally end up after that with two movies, and one prepared by the Ames Research Center, and the other prepared by the Langley Research Center, on the work that has been done on guidance and stabilization problems for this project.

I will now turn the session over to Dr. Liller.

SOLAR EXPERIMENTS, UNIVERSITY OF MICHIGAN, BY DR. W. LILLER.

DR. LILLER: I don't believe I need a blackboard for anything, so I shall stay right here.

First, telling you what we would like to do, our experiments are entirely on the sun and the solar atmosphere, because there will be certain simplification in the pointing system, presumably some of the payload space, and weight which will be needed for the other experiments will not be needed in ours. We will probably like to use this additional space for data storage capacity.

I have divided the brief description here that I am about to give you into six different areas. So let me just run down these.

First, what we want to do. We want to study various areas, small areas on the surface of the sun. These areas will have a maximum size probably of a square minute of arc. Remember now that the diameter of the sun is a little over 30 minutes of arc. These areas may contain nothing more than the quiet sun surface, and we may wish to note this for a study of the solar area from the center out to the edge of the limb of the sun to see how it varies to get information from the height distribution of these different types of radiation that we will look at.

We will also be looking at excited areas, solar flares particularly, sunspots, prominences or coronal regions which should be fairly bright in certain radiations. These areas would be selected in advance, perhaps as the satellite is going overhead, and the satellite would then be programmed to observe the particular area which has been chosen, and then the equipment will be started.

No. 2, the instruments that we will carry are quite different from the ones that you have been looking at so far this morning. Instead of having a large telescope with a small spectrograph attached to it, we will have a relatively large spectrograph with only a small collecting surface. The diameter of our collecting mirrors will be measured in just an inch or two for the most part except perhaps for the far UV and the x-ray regions. We intend to have three grating spectrometers. The first one will cover the range from 1500 to 3,000 angstroms with a photomultiplier scanning the spectrum as we rotate the grating. Secondly, a spectrometer covering the region from 500 to a little over 1500 angstroms to get some overlap again with a photomultiplier output, and the third spectrometer will cover the region from a little greater than 500 angstroms down to as far as we can go. We think this will be under a hundred angstroms down to maybe 75 angstroms, again with a photomultiplier output.

The fourth instrument of the four main ones that we intend to have will be a spectroheliograph or in this case called a spectroheliometer, which will be set on the Lyman alpha line of hydrogen at 1216 angstroms. An image of the sun will be produced in this radiation and will be monitored by a vidicon, either by command or by storage.

There will be posssibly additional small experiments, x-ray counters, for example, which do not take up much space or weight; possibly a second television receiver camera will be used to photograph the sun in other wavelengths, perhaps, than in the x-ray regions. Third, the operation of the system will be run something like this.

Each spectrometer will have two, possibly three, scan speeds. The fast scanning speed will give us a total scan of the particular wave length region which that spectrometer is intended to observe in a relatively short time, two minutes. The sun is bright. We can do this and we can get quite a bit of information. Two minutes is chosen to be roughly a tenth of a lifetime of a solar flare, probably the shortest lived event that we will observe on the sun. This scanning will run continuously as long as the sun is up above the horizon and fairly well out of the atmosphere of the earth. So this will be rapid scans back and forth during the orbiting time.

There will be a second speed, a very slow scan, one scan per orbit, that is,it will last of the order of 45 or 50 minutes, the length of time the satellite is in sunlight. It is one scan per revolution in this case.

All three spectrometers will have an arrangement such as this. A spectroheliometer will scan relatively slowly. It can be a number of seconds. This we have not looked into entirely yet. The total number of pictures which we will take per orbit depends on how much storage capacity we have. This I will come back to in just a moment.

Section 4 is on the information rates that we will be sending back and the storagewe will need. For a fast scan we estimate that we will have for any one spectrometer several hundred bits per second, information bits per second, or a total of about a million bits for a complete orbit. This is with the fast scanning going all the time. The slow scan will not give quite as much information per revolution. It will be about two times ten to the fifth information bits. The spectroheliometer, the total number of bits depends on the resolution of course that we can get, but presumably it would be of the order of a million information bits per scan. We think in terms of a picture 200 by 200 picture elements with a dynamic range of perhaps a hundred or something of that sort. We would then have,we roughly estimate, a million information bits. As far as the storage is concerned, we would like to be able to store the order of ten to the seventh information bits, which is really quite a lot. It does not seem impossible, though if we use tape, although of course the moving wheels you have in a tape recorder should be avoided, if possible. So, core memory systems or storage tubes, that is something that we have to look into yet, and we intend to do this with some care. If we can get ten to the seventh storage bits, we should be able then to take a number of spectroheliograms during one orbit,of the order of 10 pictures perhaps. We would settle for less, naturally. We may have to settle for less.

The actual procedure: We picture an operator at one of the receiving-transmitting stations on earth He would have available the most recent spectroheliogram or, preferably, if the satellite were in sunlight at the time it went over the station, the operator should be able to command the satellite to send a picture of the sun in Lyman alpha as it appears at that instant. This would be done presumably in the time which is something less than a minute, and he would then have the several decisions to make.

First, what area of the sun to point at, whether it be a flare or a sunspot or just the surface of the quiet sun, or out in the corona somewhere, and we hope that he will be able to point to the nearest minute of arc. We envision a console where he can by step command have the pointing axis of the system move a certain number of minutes of arc to the east or west or north or south, and then the operator must make the decision whether to start slow scanning or fast scanning in operation or none at all, of course. He does not have to start it at that time. So he has basically these two decisions, where to point and how fast to make the observations. Once the pointing axis is located on the sun's disc, the guiding should be of the order of one second of arc. The granulation of the sun's surface, as the Princeton photographs from the high altitude balloons have shown, shows that there is a great variation in intensity across the sun's disc, and if we are looking at the order of a square minute of arc, we would want to hold it fixed on that square minute of arc to about one second of arc.

There would also be interrogation command, too, for the information to be sent back and recorded on the earth.

Finally, listing the different commands that we should have, we should have an east-west coordinate control. As I mentioned, a step command with perhaps 60 steps of one minute of arc. This would enable the observer to go out into the corona, go off the solar disc, and observe in the x-ray or far ultraviolet regions the corona lines. Secondly there will be the same arrangement north and south. Third, a fast or slow command. Fourth, a command to start a television scan immediately and transmit it back directly with no storage. This the operator would do when he is ready to make his choice. He would push this button, the scanning would start -- first, the transmitter would be turned on, and then the scanner would start after sufficient warmup time. The fifth command would be simple interrogation.

I forgot to mention in section 4 actually that the dynamic range we would like to be of the order of ten to the sixth, ten to the fifth would be satisfactory.

I think that is all.

DR. ROMAN: Are there any questions on the solar experiment?

MR. STEINMAYER (Bell Aircraft): Dynamic range of what?

DR. LILLER: Of the order of ten to the sixth, but ten to the fifth would be pretty good for our purposes. We would like to get a one per cent accuracy on line intensities. I might mention that the ultraviolet and x-ray spectrum of the sun below 1500 angstroms will be primarily bright emission lines, fairly sharp, fairly discrete, Above 1500 angstroms it would be a dark line spectrum primarily.

MR. TRIPLETT: You mentioned pointing the vehicle to one minute of arc from the ground. You mentioned also one tenth of a second of arc. Have you given any thought to what detector you would use to give guidance to one second of arc, how this is used automatically?

DR. LILLER: Some of the pointing controls that have been developed at the University of Colorado by Ball Brothers seem to be the type of thing that would be needed. Whether this can be pushed to one second of arc I cannot say.

MR. TRIPLETT: I was wondering, could you get it effectively locked onto particular areas or whether you would have to look at a star for reference, or some other means?

DR. LILLER: Two axes control would be quite easy where you just lock on the limit of the sun. The third axis would be a little more difficult where you would want to pick up the star. I suppose it would be all right, too, to set on a sun spot. The sun spot would not move appreciably during the time of observation. I think this would probably be sufficient actually.

DR. ROMAN: Would your observation time be short enough so that you could use stars or would the motion between the sun and stars be severe?

DR. LILLER: That is something I could calculate quickly. I think it would not be that large.

DR. ROMAN: A motion of a degree a day roughly?

DR. LILLER: Yes, this would be four minutes of arc an hour, something of that sort.

MR. KERPECHAR (Kearfott Co., Clifton, N. J.): I don't understand why you must point this accurately. Will you perhaps review this point briefly?

DR. LILLER: Your question is why do we want to point this accurately?

MR. KERPECHAR: Yes.

DR. LILLER: Well, many sun spots are of the size or even smaller. Solar flares, for example --

MR. KERPECHAR: The resolution on the sun then is the determining factor, not the resolution of the equipment.

DR. LILLER: That is right. Our resolution is generally something better than this.

DR. HELVEY (Radiation, Inc.): Is it envisaged that in this observatory there will be sensors incorporated for other than electric magnetic radiation?

DR. LILLER: We have not included this as one of the primary instruments. Certainly I listed additional equipment such as x-ray counters and so forth which could certainly include particle detectors, too. I think this would be very valuable.

DR. ROMAN: I think primarily we can say that this project is not designed for non-electromagnetic radiation. Any equipment of that nature which would be carried would be definitely of a secondary nature which would go along because there was room and because it could use the vehicle. But the vehicle would not be designed to carry it.

DR. HELVEY: I could see some very great advantages to have a co-incidence in certain operations of both.

DR. LILLER: Very true.

DR. ROMAN: Any other questions?

MR. GOOD: (RCA Astro-Electronic Products, Inc.) Would you care to develop any more information on your spectroheliograms? For example, how you intend to obtain the image of the sun, what resolution you will find on your TV tube and so on?

DR. LILLER: A very nice spectroheliogram has been photographed already from a high altitude rocket by Richard Tousey of NRL. His system is quite simple and we intend to use it. It is nothing more than two concave gratings, one of which forms a slitless spectrum of the sun, Lyman alpha being extremely bright and therefore relatively isolated as far as bright radiations are concerned. It stands out very nicely. You put a circular diaphragm over this slitless image, and then a second grating puts it back together again, enlarges and focuses onto the vidicon. The actual resolving power available here is limited simply by the, or primarily by the diameter of the gratings of which the resolving power should be of the order of a second of arc or so. The ultimate resolving power will be set, I am sure, by the vidicon, itself. So if we had 200 by 200 picture elements, and we filled the sun up in the screen, in the field of view, we would have something like a third of a minute of arc resolution.

MR. HUTTER (RCA Astro-Electronic Products, Inc.): The ten to the seventh bit storage you mentioned, is this strictly storage corresponding to the spectroheliometer image, rather than additional information from the spectrometers?

DR. LILLER: This storage was total storage, the spectroheliometer images plus the scanning information that we have.

MR. HUTTER: Do you expect to break this up as to how many bits would be in the spectroheliometer?

DR. LILLER: About 90 per cent.

MR. MEINEL (Kitt Peak National Observatory): Are vidicons sensitive to Lyman alpha or are any available?

DR. LILLER: No, as far as I know there is none available, but 1963 is still a little ways off.

Lithium fluoride is still fairly transparent. Perhaps a window can be made.

DR. ROMAN: Any other questions on the solar experiment? Are there any other questions on any of the experiments?

MR. BOROUGH (Boeing Airplane Company): Dr. Rogerson, the angular field view of the spectrometer, including the optical system, will have a five micron slit?

DR. ROGERSON: That is one degree. That will be for the fine TV.

MR. BOROUGH: The spatial angular resolution with five micron slit of four inches in diameter?

DR. ROMAN: You mean what does five mincrons at the focus correspond to angularly?

MR. BOROUGH: I thought it was about eight angular seconds.

DR. ROGERSON: I think it is on the order of a tenth of a second of arc.

MR. TRIPLETT: I think it is .08 of a second.

DR. LILLER: Ten to the second.

MR. STRAUSS (Aircraft Armaments, Inc.) I am thinking now of the structure which will support this imposing array of optics and the fact that the sun will rise and set on it twice every hour or so, and therefore introduce some thermal expansion. Which of these experiments can be designed in some nicely symmetrical way and which can not?

DR. LILLER: I might say that certainly optical designs have been devised whereby you have, say, a steel supporting member which expands outwards with increase in temperature and an aluminum pillow on which the mirror sits which will expand backwards and counteract the thermal effect. In theory anyway these thermal effects on the focusing can be avoided. They may cause changes in thermal noise of the photomultiplier.

MR. STRAUSS: There are, for example, off axis objective in many cases. I wonder whether the gentlemen who have these experiments have considered this symmetry problem.

DR. DAVIS: Our depth of focus is much larger than you usually think of for these instruments.

DR. ROGERSON: In our experiment we hope very much that one side of the satellite will be constantly oriented toward the sun, and there will be then a thermal barrier, double silver layer between that and the compartment containing the optics to minimize just this problem. That will give you one hot or at least warm region where you would want to put the electronics anyhow, and one very cold region which will be good for the photocells. By cold I mean something like a minus 100 centigrade.

MR. MEINEL (Kitt Peak National Observatory): There is a problem common to all of them that has not been discussed. How do you reacquire or what do you do about guiding when your objects are not visible to you?

DR. ROGERSON: That has been a great concern to us too. We had hoped to have an anti-stellarscope that looks 180 degrees from our primary instrument plus or minus a degree. We hope to be able to find a star brighter than the ninth magnitude that fulfills the requirement for any program star we are interested in. If the earth occults our primary instrument, then the anti star can take over the activeguiding. The accuracy need not be very great, enough so that we can keep it when it comes around.

DR. ROMAN: There are other possibilities for this. There are some problems involved with the anti-star. Some other possibilities that have been mentioned are that you could use a gyroscopic reference system for this length of time. In theory if you can keep your satellite well enough balanced, you don't have to have any active guiding.

MR. MEINEL: There is a tolerance regarding that, because that is one of the obvious solutions.

DR. ROMAN: I think the tolerance regarding it is that then it simply cannot get off so far that the guiding system cannot lock onto it again and start tracking. This depends entirely on the guidance system, that you have. I know this is sidestepping the answer, but I think this is one that you can't say unless you specifically define what you are using for guidance.

MR. KIERSTEAD (Goodyear Aircraft Corporation): If there are more than one experiment in a satellite are we to understand that only one will be run at a time?

DR. ROMAN: Yes. As I said, there probably will be more than one experiment in a satellite. They will operate on a time sharing basis. I do not foresee trying to run more than one experiment simultaneously.

Any questions on the experiments? If not, then, I think we can get to the engineering parts of the program. I will ask, after a brief interval to erect the screen, Mr. Triplett of the Ames Research Center to describe some of their thinking on the engineering and to bring up to date the engineering problems on the project.

ENGINEERING ASPECTS OF THE ORBITING ASTRONOMICAL OBSERVATORIES, NASA AMES RESEARCH CENTER, BY W. C. TRIPLETT.

MR. TRIPLETT: We have heard something from the experimenters of the type of optical equipment and instrumentation needed to do this job. I would like to make a few remarks on the engineering aspects of the problem.

First of all I might mention what our interest at the Ames Research Center is in this project. We have responsibility for doing research on the various engineering aspects of the astronomical satellites. We are also in a position to act as consultants both to Space Flight Development and also the industry and thirdly we will help in evaluation of proposals and also in the writing of specifications.

(Slide)

First, this slide is to illustrate what some of the engineering areas are. They are of prime importance in this project. We have about five major areas here. This has been weighted more heavily in attitude control because I think this is one area that is unique to this particular project. Some of the requirements you have just listened to as far as pointing accuracy, the attitude control will be specialized. Some of the areas are common to other satellite systems. As far as attitude control goes, we look at various system concepts, that is, means of producing a control torque. We have listed here four possible systems, first reaction wheel control, motor driving the flywheel, which motor will produce an equal torque on the vehicle.

This also may be accomplished by a gyroscope in which the flywheel turns at constant speed, and a torque is produced by changing the spin axis of the gyro. These are two possible schemes involving momentum transfer.

A third system that looks promising both for fine and coarse control is the vapor jet system. This is a very low pressure gas jet system. It is very simple in concept.

The fourth system here is cold gas or high pressure jet system. This may be of some use in the initial stabilization of the vehicle.

Another integral part of the vehicle attitude control is error sensors. The type of error sensors used will probably dictate the type of control you can use. Here are three of them.

First is the television for acquisition and course guiding. This is in a sense an error sensor. The operator from the ground uses a visual picture as an indicator of the vehicle attitude.

Second we have the solar tracking. We also have star trackers or error detectors that make use of stars, that are being used in the experiments.

The third area that must be considered, in fact is one of the most serious areas I think in specifying attitude control, is the effect of external disturbances on the vehicle. We know well of torques due to the earth gradient, due to solar pressure. Those appear to be most significant. We also expect torques through the earth's magnetic field, perhaps due to the effect of the earth's atmosphere on the vehicle. These put very strict restraint on the system to be able to track to the accuracy desired. In that sense it means that the vehicle must be balanced in order to eliminate the earth's gravity torque. It must be earth symmetrical as precisely as possible. Consideration must be given to locating the center of pressure, solar center of pressure, coincident with the center of mass. I think those are the two outstanding effects.

The fourth concern in the attitude control system is the dissipation of angular momentum. If you use a momentum transfer system, such as A and B, then some means must be considered to get rid of the momentum that is finally stored on these controls -- there are a number of ways to do this. Either by use of a solar sail, or a sliding weight has been suggested, it means changing the inertial characteristic of the vehicle. It is possible to use a jet system, apply an impulse periodically. I think in regard to the experiments, the time this operation should take place is during the time, generally viewing stars, at sometime during the orbit the star will be occulted. This will be the logical time to remove momentum from the control wheels so that there won't be any undue disturbance during observation periods. This implies, then, that the control wheel should have enough capacity to last for one orbit without saturating.

We will come back to the attitude control a little bit later. We will go on to the other areas, engineering areas.

The second area is the power system. We have thought mainly in terms of a solar cell system and storage batteries. They represent a state of the art development. They are available. We have some experience with how they work. Solar cells appear to be very reliable and reasonably efficient as long as they can be kept cool. Storage batteries on the other hand, if we assume a vehicle lifetime of one year and charge-discharge cycle every orbit, the vehicle will be in darkness part of the time of every orbit. That means that the storage batteries must have a cycle lifetime of about 25,000 charge-discharge cycles.

Now, nickel cadmium storage batteries do have this potential. However, the charge cycle is shallow. In other words, the charging rate is not too high. However, experience has shown that a significant number of these might fail even below a thousand charging cycles. So there is some problem there on reliability of storage cells. It may mean in essence an over-design of the whole power system.

A third area is thermal balance. Here are some very unique problems, not only in regard to thermal gradient across the optical systems that were mentioned previously, the fact that some types of photocells, photo detectors, must be kept very cold, perhaps minus 100 degrees, or something in that order. Storage batteries, other types of electronic equipment, must be warm. Solar cells must be cool. Their efficiency deteriorates very rapidly with increase in temperature.

We have a number of thermal problems. Thermal problems are complicated; even though you have a vehicle that points toward the sun, it will be in darkness part of the time. Apparently the ratio of time in sunlight and darkness will change during the year, depending of course on the inclination of the orbit. At one time it may be 60 per cent of the daylight, at other times of the year 80 per cent daylight.

The fourth area here is communications. This includes radio command systems, both receivers, transmitters, data storage system, and also data transmission systems. Mr. Foster will have a few words to say on this subject so I will skip right over it.

The final area is in the layout of the vehicle. Here we are concerned with the final vehicle that may include more than one experiment. It is restricted in weight, it is restricted in dimension bythe size of the booster. It must include all the other factors we have considered. It may be required, for example, in regard to the solar cells in order to handle the thermal problem, it may be desirable to put the solar cells on paddles, rather than as part of the vehicle. In any event, all these considerations must be worked out in arriving at a final vehicle layout.

(Slide)

These are some of the problems we have been trying to look at at Ames. So far our work has been mainly devoted to attitude control systems. We have been looking at various types of systems, various concepts. We have tried to evaluate them on this basis.

First of all, we must have a dynamic performance, even a steady state performance that is consistent with the requirements of the experiment. If the system can meet these requirements, then we would like to evaluate and compare different systems on the basis of these other factors. That is power, weight, reliability, and availability. We may lean towards things that are available today rather than something that is in the state of development and may or may not be ready by 1963

(Slide)

Now, in order to really evaluate control systems realistically we have to set down some requirements. I don't look at these really as requirements, but more as ground rules under which we must operate. We can set down some of these, it gives us a common basis for comparing different types of systems, different concepts. So we look at the control problem as really in three phases.

The first phase is the initial stabilization. We are considering an initial tumbling rate as high as one degree per second, which is quite high. We are assuming that this initial stablization system -- it may be a jet system using a gyro reference -- should be able to stabilize the vehicle rates to within .002 of a degree per second. This seems within reason on the jet or gyro systems. We have considered/with the same operation that the vehicle will be oriented in space so that the solar cells will be pointed toward the sun/within approximately one degree. In this position, then, the vehicle would be essentially stabilized in space where its attitude then could be determined from a remote control television picture, and then from this point the acquisition of target star could be achieved.

Now, as far as acquiring a star, one of the requirements specified has been a maximum slewing rate of 180 degrees in five minutes. The ground observer by remote control can acquire any star visible within the time that the satellite takes for one pass over the ground station. This is really quite an excessive requirement, excessive from the standpoint of importance required. We also were thinking in terms of remote control pointing the vehicle to the plus or minus 15 minutes of arc. On some experiments, particularly the solar experiment, they would like to have this done to one minute of arc, but this was, I say, one set of ground rules.

(Slide)

The third phase of control would be the automatic control. We are looking both at coarse control and fine control. Here is a coarse control that would really be a continuation of television acquisition. After the error got down below a certain size, then an automatic mode would be switched on in order to bring the error down to the limits of the coarse control system. So here we were thinking in terms of a detector that would sense initial error as large as 30 minutes of arc. This coarse system would provide a final vehicle pointing of one minute of arc.

You will recall that in some experiments this is perhaps all the accuracy that is required so that a coarse control system could do the whole job. Again during this phase of the operation, we would still have this residual .02 of a degree per second vehicle rate that will be carried through. We would have external torques of nearly one hundred dyne centimeters. This would be what we think is an extreme figure, but it is a sort of torque that could be expected with center of gravity -- I should say moments of inertia of the vehicle equal to within one half of one per cent, and also with center of solar pressure just a very few inches from the center of gravity of the vehicle. You could get steady torques that would act in one direction for longer periods of time on this order of magnitude. This is the total angular momentum of the vehicle if it were slewing at the rate of 180 degrees in five minutes. I don't think that figure is really significant here.

The final phase of the control would be the automatic fine control. We are considering an initial pointing area of two minutes of arc. There would be some overlap in fine and coarse control. We are considering ultimate pointing accuracy of one tenth of a second of arc. Again the vehicle could have initial rates as high as .002 degrees per second. It will be subject to external torques of 100 dyne centimeters. One of the requirements -- I think this is the more serious requirement as far as pointing accuracy, even more serious than one tenth of a second of arc -- is to hold down the steady state or the drift rates due to this 100 dyne centimeter torque. Depending on the nature of the system, if it has an effective integration when the torque is acting on it, there will be a steady state pointing error. To keep the limit within this tenth of a second of arc we have to specify that this steady state due to the 100 dyne centimeter torque will not exceed five seconds.

We have also specified that the saturation time, that is, if a reaction wheel system is used, should be 100 minutes.

There is one other interesting thing that was mentioned earlier, the aberration of light. That also puts a stringent requirement on the system. In view of large disturbing torques that requirement is of secondary importance. If these torques could be reduced, and they may well have to be reduced in order to develop a system that can come anywhere near meeting these requirements, and then it turns out that the aberration of light will be one controlling factor to be able to handle a line of sight rate of some .3 of a second of arc per minute, that may be one of the most serious constraints.

(Slide)

I just want to mention briefly an example or two of the types of systems we have been looking at at Ames Laboratory.

First, these two are both reaction wheel systems. The first, to go through the diagram, we have an error sensor which detects the difference between the true line of sight and the target star, and the actual pointing direction of the vehicle. This may be modified by some passive network. The signal drives the motor wheel, producing a torque, subjects it to external torques, the torque is integrated to produce a integrated rate, integrated again to get vehicle attitude.

We have also considered a tachometer feedback possibly shaped by a network. This type of system does promise reasonable dynamic response, depending on the types of networks you have here. We are in the process of examining various types of networks in order to provide sufficient damping and to minimize the steady state.

The second system we might consider is very similar. Except for the tachometer feedback, we do have an integrating gyro. This from the standpoint of complexity might not be so desirable. It is an additional moving part but if you can consider a perfect rate gyro, then this offers an ideal type system because it gives you all the performance you desire. So it is what we might consider a standard system that we can use as a basis for comparing other systems.

Now, this type of system does have one additional advantage. When the target stars are occulted the loop is broken right here, the inner loop will operate and the vehicle will remain phase stabilized during this period of time. A system of this type can hold a vehicle to within a minute of arc for as much as 30 or 40 minutes. So there is one additional advantage you get in turn for additional compexity. When you consider this type of system, it may be possible just to remove the motor altogether to use the gyro, use the gyro in two modes of operation; use the gyro to provide torque for the vehicle and also use a gyro as a stabilized space reference. This is another area worth looking into.

One more type of control system we looked at a little bit is our vapor jet system. The nice thing about these systems is their complete total simplicity. There is no moving part other than the valve. We have the same sort of error sensor. We modify the error signal and put it through a logic network and the signal out of the network operates the valve. That is all there is to the system.

Here we have the same torque, external disturbances acting on the vehicle. This is essentially an on-off type system. It uses just one thrust level. It has two modes of operation. For large errors, we have the continuous mode. We have a steady thrust. Here we have a switching object. Here is the plot of the error versus error rate. We sense both of these quantities. This is a switching logic network that follows this form. Whenever the vehicle is out on this side, you get a steady thrust until it crosses this line. Then the thrust is reversed. It is essentially a bang-bang type of system. When the error rate gets down to within certain limits then the mode of the system switches, we have a pulse mode where a standard size pulse is triggered. In this case we set limits to one eighth of a second. Whenever the error reaches that value, you trigger the pulse. You end up in a steady state with a sort of limited cycle operation. For example, we have considered the period something on the order of 50 seconds. The pulse would actually fire every 25 seconds. If external torques are applied to the vehicle -- this is no external disturbance -- you get the same sort of pattern except it would always tend to go back to one side. Fuel economy for this type of system, we considered water vapor, specific impulse of about 40 and still for each channel this type of operation involves about two pounds of fuel per year. It is quite economical in fuel.

There are two problems associated with this type of system. First of all, any use of fuel will change the balance, inertial symmetry of the vehicle, and secondly what may be more severe is what will happen to the water vapor that is exhausted. Will it go in the form of a cloud around the vehicle? If so, it will destroy the experiment you are trying to accomplish. Until that question is answered we certainly could not consider using this type of system but it is interesting. It will have many applications.

(Slide)

I have one more slide to illustrate the pieces of experimental equipment we have been using at Ames Research

Laboratory to evaluate systems. We have developed a missile simulator here in which we have a large platform which simulates the missile, the satellite. We have this platform floating on an air bearing. You can see it on the middle of the picture. There is a supply of air which actually floats the ball, the whole platform is floated on air. It takes about 30 pounds of air pressure through an orifice, in the bottom of this disc to float the whole works. The table weighs about 200 pounds. So we have virtually a friction free table.

We have here a little telescope with an error detector and photodetectors on here. So we have a two axis control system.

Out of the view of the picture there is an artificial light source. So we can simulate the operation of an entire control system. Now, what we had at the time the picture was taken was a reaction wheel system. We do have one reaction wheel motor here that controls the vehicle in pitch. We have another reaction wheel here that controls -- we have enough pendulum effect on this platform so that there is no control on the third axis. There is some feeling for the type of control system, the stability problems we encounter in using actual hardware. We also get some feeling on the type of response we get from various error detectors. We are really just getting under way in this area right now.

I think that is everything I had. I know that John

Foster would like to say a few words about the communications area. So I will let him take over from here.

PRESENTATION BY JOHN FOSTER

MR. FOSTER: I really don't have too much to say on the communications. We have only looked into it to a large extent in association with the control system work. However, I will make a few observations here of what we have at least thought about.

The data system really will have to cover four different categories of data. First are the ones associated with the vehicle performance and command. This will be the state, for example, of the inertia wheels, whether it is saturation or not, various commands that go up to operate the optics and returning data that indicate that these commands have been performed, this type of data which will essentially be a rather narrow band type of telemetering.

Then there is a second type which is the acquisition TV in various of the experiments or on the platform itself there will be a TV system that looks at either a 10 or 15 degree field, and then finally in some cases down to one degree field for acquisition. This picture will have to be sent down and displayed properly to the operator.

The dynamic range of the light on this particular TV will not be as great as some of the TV required for the experimenters. Therefore, the band width requirement on this aspect of the TV problem may not be too severe.

The third thing, our type of data will be that required by a majority of the experiments noted today which are readings from photo tubes on the spectrometer type of experiments. In general this can be handled by a rather narrow band telemetering because it is an accumulated count deal, and the scan rates in general are quite long, so that the rate of accumulation of data is not high, and you have sufficient time to send it down by a rather narrow band width telemeter.

The last category is the experiments which have a TV scan type of data in their accumulation, such as that explained by Dr. Davis. I will go into it a little bit, what we have done about it. Let me say the narrow band data from the platform and from the experiment I don't think should present any real problem, so I won't dwell on that. The acquisition TV can be relatively lower accuracy than the data, probably should be reasonably easy to get within an available band, within limits of the available stations. So this leads to the last category which appears to be somewhat the tougher and with due apologies to Mr. Davis, whom I have not had a chance to talk with here, I would like to make some statement on which he can argue with me or not, and they may be incorrect so far as that goes. But in looking at the requirements of the TV scan associated with that type of program, that was where you made the whole sky sort of essentially a two degree sort of segments.

One of the bugaboos here is that the large dynamic light range that would be required for the experiment plus a reasonably high accuracy, a tenth to a magnitude star, which in a preliminary aspect was quoted at 10 per cent accuracy, this was based really on a tenth magnitude star accuracy, this ten per cent accuracy data. The thing is if you look at a 500 by 500 line frame, and you want to scan and send this data down in one second, you can come to a first low band width of 250 KC. This is simply multiplying the lines by the resolution across the line, but our feeling is that really you can't with this dynamic light range and accuracy requirement get by with that narrow band width.

Then you look at various other schemes, you could code each of these, each and every one of these possible 500 by 500 points. There you arrive at something around two megacycles band width. Well, we won't have a capability for that. So some place in between here will probably be the true case.

One other thing I would like to throw into this, possibly for consideration of the Smithsonian experimenters, is that you might look at this and say, well, there may be only 50 stars maximum in any one of these frames, and you say well, why worry about all the 250,000 picture elements? Why not just worry about the ones that have information on them? If you detect these, break them down into a binary code and store them with each individual position of course as the thing is scanned. From that aspect it turns out you can get down very low band width; it turns out to less than 3 KC for one second of scan of frame range.

A little problem comes up. There will be concern with luminous areas which are not contained in these 50 star points. So we thought possibly a method might be, a hybrid method might be evolved where you were scanning and storing the information from these 50 stars, the luminosity areas may be of a lower frequency enough that they can be handled with a rather narrow band width. At the end of this scan then you send out the ooded information of the 50 point sources, the stars, and maybe get by with a reasonable band width but I believe there will either have to be a method of this nature or possibly a longer scan rate to really keep it within the band width requirements. This is something that we have not really looked too far into except I think it will be somewhat of a problem. That is really all I have to say, unless there are any questions on that aspect.

MR. CHENG (Hughes Aircraft): Is there any reason why this must be transmitted in one second, other than the fact that your experiment will take longer?

MR. DAVIS: There are four television tubes. The one second was arrived at by using half of the motion time for the transmission of the signal. There is no reason why it could not be prolonged.

MR. CHENG: In other words, if this proves to be one of the nastiest problems of the whole experiment that you are trying to do, there is a possibility that you might compromise a bit on this and say well, we will reduce the band width requirement by going perhaps to two seconds transmission?

MR. DAVIS: Two seconds would not bother us. More than two seconds would actually start slowing down the experiment. We would start looking at coding methods as a possibility.

Dr. Dursey, how long do you think we could afford to stretch out the scan?

DR. DURSEY: Excuse me. I would like to tell you in your computation about the 150 kilocycles, actually if you consider just an analogue transmission, go to one half, 125, because if you have a 250,000 picture element on your picture and you consider the maximum presently involved in the situation, you have a complete aperture element followed by a complete black one. You actually have 125 kilocycles per second.

MR. FOSTER: I must admit that it is rather difficult to come to a band width requirement. We find we are not alone in not being able to come to exact figures in this. When you bring in the factor of your required accuracy in the dynamic --

DR. DURSEY: Absolutely. You must shift from an analogue type of transmission to a coded type. So the consideration of band width is completely changing. You are perfectly right. What I imagine Mr. Davis was pointing out was that with a normal TV system it is meeting with some certain number of letters like a normal TV system is doing, you may go down to 125 kilocycles per second band width in one second.

MR. FOSTER: I am not quite sure that I --

DR. DURSEY: You take, for instance, a quite usual television system with 30 frames per second, that is corresponding to four megacycles per second. How is the band width computed, just taking a certain number of picture elements, for instance, 500 lines horizontal resolution and 500 in vertical resolution. You have 250,000 picture elements and actually if you want to scan 30 times per second, you reach 50 times 125, so 4.5 megacycles per second. That is no consideration here about accuracy in the determination of the levels. That is the way I imagine the Smithsonian has followed. If now we have for every picture element to consider carefully a certain number of letters we have to visualize our picture transmission, and so we may go to a much higher, much wider band width.

MR. FOSTER: This is the thing I was bringing up. It is something that will have to be considered. I think in the early specifications, things of 150 KC and 250 were pointed out as probably covering the experiment. I think this is not true. It will have to be looked into more thoroughly.

DR. DURSEY: Who designs the experiment to fit into whatever band width you give us?

MR. FOSTER: Are there any other questions?

MR. COLLINS (Page Communications): Considering the aspects of solar pressure and/ moment of inertia and all these things mentioned previously, is it possible to consider the use of six foot parabolic antennas on this vehicle?

DR. ROMAN: On this vehicle?

MR. COLLINS: Yes.

DR. ROMAN: I think this is for someone else other than me to answer. Do you have any answer on that?

MR. TRIPLETT: I think antennas is one thing we have not mentioned. I think that can be a serious problem from what I gather. The type of antennas you need for transmission still has the direction in which they have to be moved.

MR. FOSTER: Actually both the requirements of the center of radiation pressure and the CG, as you probably figure, will be very stringent if you have to move any external--

MR. TRIPLETT: You agree there is a problem, but you are not sure of the answer?

MR. FOSTER: That is about it.

DR. ROMAN: Any other questions?

MR. BENEDIKT (Norair, Northrop Corporation): (Remarks concerning sputtering inaudible.)

MR. McDONALD: When you quoted ten watts for power for transmission, Dr. Davis, what type of antenna did you have in mind?

MR. DAVIS: That was a sixty foot paraboloid on the ground, and isotropic radiator for the satellite.

DR. ROMAN: I would like to thank you for your remarks on sputtering. We have not gone into the environmental effects at all here today largely because these are being looked at elsewhere in the space problem and I felt that it was not for us in astronomy to tackle them as well. However, you are right that sputtering is something that will have to be investigated.

MR. CHATKOFF (Minneapolis-Honeywell): Since your driving torque is the function of the orbit, is there any method of keeping the orbit eccentricity down?

MR. TRIPLETT: The measurements we have made assume a circular orbit. The only way to reduce these torques, the main thing is to make the vehicle symmetrical. There is a problem. We have thought in terms of symmetrical to one half of one per cent. It is questionable whether you can even measure moments of inertia that accurately. That is the problem, to build this vehicle as symmetrically as possible. The other important one is the solar pressure.

MR. CHATKOFF: I was thinking of the eccentricity of the driving function in the unsymmetric inertia, the fact that your angle of gradient rotated.

MR. TRIPLETT: That would be an additional source of trouble. You would have to minimize the total effect of all the sources, at least minimize it down to the point there are a number of things that will be relatively uncertain.

MR. CHATKOFF: You have made no attempt to make the orbit perfectly circular?

DR. ROMAN: What do you mean by perfectly circular?

MR. CHATKOFF: By having a correction device on board to take out any eccentricity.

DR. ROMAN: I think we will probably depend on the vehicle that gets it up there to get it in as good an orbit as we can.

QUESTION: This type of orbit would be elliptical and not circular so it does not lead to saturation.

MR. TRIPLETT: That is right. It is the circular torques that will cause the trouble.

DR. ROMAN: Any other questions on either of these?

MR. CHATKOFF: Mr. Triplett showed a picture on a screen of a platform. I believe I got the impression that this was an experiment for stabilizing the vehicles by using your vapor jet and you had a sensor. Will you describe the sensor?

MR. TRIPLETT: The sensor is essentially a pyramid type. It is a four sided pyramid. So the light impinges on this pyramid. We get a signal. We have four photocells, one looking at each side. Then we sum the outlets of these two cells. As the light moves over to one side, we get more light in one cell than the other.

MR. CHATKOFF: You imagine you may utilize an optical sensor for control in orbit where you might be subjected to drift?

MR. TRIPLETT: When you get to the actual vehicle you may want to use a single cell, sharper arrangement. You may not want -- the sum of the signals of the two cells, yes.

MR. MITCHELL (Boeing Airplane Company): In this simulator and in your other control calculations have you included in your control logic the inertial cross couplings or have you done this on the basis of isolated action?

MR. TRIPLETT: We have looked on an analogue computer, at the effects of cross coupling due to gyroscopic effects, and also due to the product of inertia effects. They appear to be of secondary importance, at least to the extent where we think you can design a system based on single axis analysis.

MR. MITCHELL: Even systems to the accuracies we are asking for now?

MR. TRIPLETT: Yes, I think so, because the rates are awfully small, very small. I think you can specify the parameters.

DR. ROMAN: Is there disagreement here?

MR. CHENG (Hughes Aircraft): According to the preliminary requirements you mentioned as far as possible the present minitrack facilities are to be utilized for this experiment. Now we have looked into some of the gains from the antennas. It does not look like it is up to the value that will be needed for a broad band reception of data of the type Mr. Davis wants to have. We just wondered whether any concentrated changes are going to be made for the minitrack antennas and possibly receivers.

DR. ROMAN: I don't think it is in the budget at the moment. This other matter will be gone into in I hope rather appreciable detail before much longer. We will be able to I hope come up with an answer as to whether we can use a modified minitrack or whether we will have to go to something else.

MR. KAMM (Convair Aeronautics): You said you will not be using polar orbits because of the Van Allen belt. Is there any decision on what orbit you will be using?

DR. ROMAN: In our preliminary specifications we started 500 mile circular and inclination 30 to 35 degrees. I think we could drop down to 450 miles. We don't want to drop much lower because of air drag. We don't want to go much higher because of the Van Allen radiation.

QUESTION: In what manner does it affect you?

DR. ROMAN: It gives you noise in your photocells which are the same as dark current noise, for one thing.

QUESTION: Doesn't it require relatively few grams of shielding?

DR. ROMAN: Yes, but the shielding will shield the light.

DR. KUPPERIAN: That depends on what signal to noise ratio you want. It requires quite a few grams to completely get rid of it. On a single photon you start getting x-ray. You have to shield then.

QUESTION: The absorption cross section in the cesium film is low enough.

DR. KUPPERIAN: It could reduce it to some level. The question is what is the level. At the moment we are getting new surprises from the Van Allen belt as the data come in. Sometimes it gets lower, high effects appear at a low altitude. I don't think we are going to be able to avoid it anyway, even if we stay below 500 miles.

DR. ROMAN: I think we had better break for lunch and reconvene at two. If there is an interest in continuing the discussion of this aspect of the problem we can, and then we will get into the administrative side of it, the management side of it. Also, at the request of a number of people in the audience, I have written in order on the board the names of the speakers this morning.

(Thereupon at 12:45 p,m., a recess was taken until 2:00 p.m., the same day.)

- - -

AFTERNOON SESSION 2:15 P.M.

DR. SCHILLING: Good afternoon. As one of you remarked to me a few minutes ago, all the paying guests were here at two o'clock. Our panel were taken to a luncheon meeting, and I am waiting, myself, for an explanation. I will turn it over to Dr. Roman.

DR. ROMAN: No explanation except we spent too long talking before we ordered.

I should like in starting the program this afternoon to take up where we left off this morning, and find out if there are any more technical questions on any aspect of the projects, any of the experiments or on the stabilization or communication.

MR. MEINEL (Kitt Peak National Observatory): I would like to ask a question of Mr. Triplett. He mentioned the cross coupling was a minor term. I wonder if that is really so. Suppose you have three orthogonal coordinates and only one inertial wheel as angular momentum, you slew that package 90°. The momentum is 90 degrees out of phase. You have to take all the momentum and feed it in here to keep your package stable.

MR. TRIPLETT: I was speaking from the standpoint of fine control. The only time you get any serious cross coupling is if you allow fairly large rates about the optical axis. You see, well,maybe the only thing controlling that is the solar orientation, controlling motion about the axis. As long as you can keep those rates low, then cross coupling, at least for fine control, is rather negligible.

MR. MEINEL: There is this problem when you move from one object to the next.

MR. TRIPLETT: Yes, slewing control. You must consider that. It might be advisable to slew one control at a time. You can get into some messy cross couplings.

MR. SAVEDOFF (University of Rochester): You mention plus or minus one degree on the solar batteries. Doesn't that limit the area the telescope can look at rather significantly?

MR. TRIPLETT: I think that was for initial stabilization. You would like to get the vehicle, one side pointed toward the sun or the side that has the solar cells pointed toward the sun to activate the power station. I think any experiment would allow more tolerance in one direction, perhaps up to 45 degrees, during observations where you can get 70 per cent effectiveness out of the solar area. Otherwise, you would have to orient the cells with the structure.

DR. ROMAN: Any other questions?

MR. BAXTER (Ryan Aeronautical Company): Have you considered the solar concentrating type of APU source as opposed to the cell type?

MR. TRIPLETT: To my knowledge we have not. I am not familiar with that type.

MR. BAXTER: I was wondering if there was any objection to this type or would it not be considered such a good idea?

MR. TRIPLETT: I would say there is no objection to any type at this time, anything that appears feasible.

DR. ROMAN: I think the biggest question is to increase the problem of radiation pressure. However, if that can be solved, I don't think that would be any objection.

MR. BAXTER: This would be the main consideration whether or not there would be any unbalance due to radiation pressure.

DR. ROMAN: That and of course reliability, and ability to supply what you want.

MR. MITCHELL (Boeing Airplane Company): Mr. Triplett, have you considered in that simulator of yours the viscose drag of the air bearings? Isn't this significant in the control?

MR. TRIPLETT: There is some drag. It is awfully small. I think with this type of equipment a more serious problem is what we call turbine torque. Any viscosity of the air around this ball -- the thing wants to rotate. This can be minimized by very close machining, very carefully polishing of both the ball and the seat it fits in. Still you have those to contend with. In fact, they can be quite large as compared to the 100 dyne centimeters we are talking of here.

MR. GILLESPIE (LRC): On these turbine torques we have found that we could minimize this effect by tilting the base to the point where the ball would no longer rotate. I think this would appreciably reduce the torque in the ground simulation test.

DR. ROMAN: Any other questions? In that case I want to make a few remarks in regard to the thing we have been talking about.

One item you might have noticed is that each of these experiments has been approached from a somewhat different angle. Some of them have come up with specifications which are not in complete agreement with the preliminary specifications and some of the specifications are slightly contradictory to one another. On that my answer is that there is not any really right or wrong about these at the moment. We are all in the stage of planning and trying to find the best way of attacking the problem. The preliminary specifications were written to try to give an idea to you people of the sort of things we had in mind. Some of the requirements are firm. In other cases, though, we could afford to back down a little if it turned out that what we were asking for was impossible, or if not impossible, at least impractical. There are still other cases where we would be willing to trade one type of behavior for another. To take a very simple example, in the television finder system, the preliminary specifications indicated we wanted a 15 degree field, we wanted to see six and a half magnitude stars. As it turned out, if the 15 degree field was too large on which you could get a television camera that would go to fainter objects, I think we could live with a two to three degree field if we went to perhaps ninth magnitude stars. I give this as an example of the type of trade off we would be willing to consider.

In the case of the contradictory approaches of the various experiments this is something that will have to be ironed out, I think all of us are still at the stage of doing planning and trying to find the best way. Eventually as far as possible we will have to find one common method of attack. We have approached this problem with the idea we would not design a new vehicle for every astronomical experiment, and I think we are still working on that plan.

In addition, as I mentioned earlier during the question period, we are not planning to devote an entire vehide to a single experiment. This means that the experiment will have to be made compatable with a common vehicle.

I have one other thing I would like to mention at this point. That is the specifications, the preliminary requirements for the optical detectors which most of you picked up at the door this morning. You may not have had time to look at them. I think a word on those is in order.

We have said that the optical detectors will be the province of the experimenters. I am not going back on that statement with the exception of the television finder system of course. However, a number of you have been asking me what type of detectors will be used in this problem. Since the light levels and wave length regions are different from those which we think about when we talk about electronic devices, Mr. Dunkelman, a staff scientist for optics and detectors, has prepared the preliminary requirement to give you people some idea of the types of systems which the experimenters and we will be interested in.

I think with those comments, I will turn the rest of the afternoon session over to Dr. Schilling.

DR. SCHILLING: Thank you, Dr. Roman. I think we are getting now into the presentation where we thought you may have questions of a general nature, including controversial questions and perhaps some questions of what comes next.

Let me mention again that today we are talking about a second generation of experiments. I don't think I have to define second generation experiments. If you want some definition, something like we have more problems than are solvable at the moment.

Let me start this off with amplifying one of the remarks that Dr. Roman made in her introductory speech. Dr. Roman mentioned that the preliminary specifications which were prepared by Ames Research Center and sent out a while ago talked about the Vega vehicle. The Vega vehicle is a two stage vehicle to place the satellite into orbit. As Dr. Roman pointed out, no decision has been made as to whether the specific stabilization platform vehicles for astronomical labs will utilize the Vega or the Agena B. The capabilities as far as satellite orbits are concerned are just about the same, so it might well be that we will use Atlas Agena B.

According to this preliminary quick look, which we have had, this would not change materially the specifications which are in your hands and anything which you heard today. It might affect, if anything, the time, when we talk about the first launching. You heard throughout today 1963. We might as well say the end of 1962. We don't know. I think I don't have to explain where our budget stands. You all know that as well as we do.

QUESTION: When will you buy the astronomical platform?

DR. SCHILLING: As soon as we have some money.

QUESTION: Do you think it might be in 1963, '62 or '61?

DR. SCHILLING: As Dr. Roman pointed out, the planning for test projects is not just budget alone. We have other problems which could not be solved much faster even with cash. So I think our planning will go ahead for 1963 without specifying at this time whether it will be launching very early in 1963 or very late. This may change both ways. I can not see that it will come much earlier than the middle or late 1962 if we are optimistic. Could I have some comments from the panel, if anything earlier than 1962 would be realistic?

I think that we have a general shaking of heads.

QUESTION: Launching in 1962 or procurement in 1962?

DR. SCHILLING: We are talking according to the specifications about launching in 1963. Perhaps to amplify this a little bit, at this moment we have funded some preliminary development contracts on a mostly study basis. Moneys that have been funded on the observation astronomy program have been in the general area.

DR. ROMAN: There is a little bit going on in that area. There are also several other contractors, some in optic.

DR. SCHILLING: Forgetting the procurement of the booster system, Vega or Atlas Agena B booster systems, when we are talking about procurement, we are talking about the next fiscal year as the first possibility for large scale development and eventual procurement. I want to throw this open to general questions on this aspect. Are there any other questions?

MR. BAXTER (Ryan Aeronautical Company): Is there any particular process we can go through in order to interest you people in specific ideas? Whom would we see, for example?

DR. ROMAN: I think I have been talking to a number of you people individually, and very many of these same questions have been coming up. When can we do the funding, and what. I think I have told most of you the same story that we cannot do the major funding this fiscal year. However, we do have some money for small study contracts. These I emphasize will not cover the whole system. They are not intended to be that large. I do have a list here of areas on which I think we might consider study contracts. I will read this list quickly.

The course orientation and slewing system, that is, how you get from one region of the sky to another and settle down there.

The fine stablization system. I think this is one we have all gone through enough. There is no particular difficulty about it.

Means of balancing the satellite in orbit. It has been brought up several times that the satellite is going to have to be accurately balanced. There have been questions raised as to whether you can measure balance ~~into~~ one part in ten to the third while the vehicle is on the ground in a strong gravitational field. I think we might be interested in looking into ways in which we don't have to do it thoroughly from the ground.

The effect of magnetic and gravitational fields. This has come up several times today. It may be that there has been enough work done on that already so that no further work is needed.

An acquisition television system, a finder telescope.

A backup system for this. I feel we would be rather unhappy to pin the whole experiment on a television system. We would like some other way of finding out where the telescope is pointing or directing it to another part of the sky in the absence of a working TV system.

The data storage system has come up. In particular there I am thinking about the storage for experiments such as the Michigan experiment which Dr. Liller described,, and the experiment which Mr. Davis described. I have already covered the detector field. Eventually we hope to prepare some preliminary specifications in the area of communications telemetry and data handling similar to those which we have distributed in the other two fields. These have not yet been prepared. I imagine it will be a month or two before they are. These are areas and the types of areas in which we think we would consider study contracts.

As to when we will request these, I think you will be hearing from our procurement department in this matter. This would go through them, and all of you who are here today, who have signed cards and let us know that you are interested, will receive any information on that when it is distributed.

For the sake of the completeness, I might mention two areas which have been brought up several times which I do not think we wish to pursue very strongly at this point. One of these I have mentioned already, the effect of space environment on materials and components. The other is the power supply. The reason is not that I do not feel that these areas are important. They obviously are highly critical for the success of this project. However, they are also critical for the success of many other projects, and there are rather large programs already under way investigating the possibilities and the problems in these fields.

DR. SCHILLING: Thank you. Are there any other questions on this subject area?

MR. REICHENBACH (Kearfott Co., Washington, D. C.): Can you outline how NASA intends to organize the system and subsystem? You have one prime contractor that will manage the subsystems?

DR. SCHILLING: I will again ask Dr. Roman to answer, but along with some introductory remarks. Some of you may be familiar with our present practices which go both ways. Normally NASA retains top management control for all its projects. Prime contracts have been let, like, for example, in our Able Project where the prime contract is let with the missile division of the Air Force with the Space Technology Laboratories. That is one instance where the Space Technology Lab is engaged in the contract.

We have another way. It goes to the Space Flight Center where we are building up, taking scientific capabilities as well as supervisor capability, we will assign various system management responsibilities.

As to the specific project here, no final decision has been made, and will not be made for a while. Dr. Roman may have some specific comments.

DR. ROMAN: I don't think I have very much to add to what already has been stated. The decision has not been made as to whether we will do the major system management in-house or in industry. However, I think that a large share of the contracting, a large share of the work other than system management will certainly go out to industry.

DR. SCHILLING: At some later time we may invite complete overall system management, but as has been pointed out, this has not been decided. System management control will be exercised by our staff at one of our centers. This may be Ames Research Center, Langley, maybe in cooperation with a small task group and a project manager.

MR. STEINMAYER (Bell Aircraft): Can we pursue that a little further? When you get to putting your hardware together, am I right in thinking that you people will probably subcontract the actual joining of the systems into one package and check out to some group?

DR. SCHILLING: That would appear very likely. In other words, we have various possibilities. To answer more precisely, normally in experiments we can talk about field packaging, where it takes an upper stage and put a payload on top. This payload comes from all kinds of places, various universities contribute experiments and so on. We usually have this payload packaging done. At this point, the Army Ballistic Missile Agency has done quite a bit with us in the Juno Program, and is doing it now. Again in our Able Project, it was done by STL. It is very likely that some of these things can be done by the Space Center. When we get to the second generation experiments like this one here, when we talk about experiments, these are not small projects any more, but we have to talk about payload systems.

In addition to the instrument which our own Center will prepare actual assembly of the complete vehicle and launch may be by the Army, Air Force or Navy or through mutual cooperation. It is very likely we will look for a contractor for everything which is above the basic booster system, and ask for complete assembly. However, we will not ask this agency to do the subcontracting for scientific experiments.

This is a long answer. Are you thoroughly confused?

DR. ROMAN: One thing we have been thinking about in this is that we might like to reserve the right to specify subcontractors in certain areas. This would of course apply to the experiments and you have mentioned it might apply to the telemetry.

MR. STEINMAYER: Pursuing that one step further, assuming you would have some contact do that for you, would you care to make any non-definitive comment about what that contractor participation might be in the space program?

DR. SCHILLING: Instead of answering in the future let me go back and give an example. If you remember the Pioneer flights, something similar was in effect, namely, where ABMA did part of the launching, as I remember, and some of you have been involved, I think the responsibility after the vehicle was up a certain altitude. Such an arrangement might be possible. This of course depends on the capability. Also a mutual arrangement. There may be a possibility of two or three contractors joining together. Are there any other comments or questions along this line?

MR. TURNER (Republic Aviation Corporation): What do you visualize would be the ultimate funding to get seven vehicles into space in 1963?

DR. SCHILLING: Orbitting as astronomical observatory projects?

MR. TURNER: Yes.

DR. SCHILLING: I think a comment was made that our panel was probably thinking of about one launching a year, is that correct.

DR. ROMAN: I don't think we actually stated that. That is what we would like to see.

DR. SCHILLING: As long as it is not one every month, I am happy, but to give you a figure would be really just artificial. Of course, we have gone through budgeting, we have gone through detailed budgeting, but with so many problems unsolved, so much depends on when and how expensive are the solutions. If I throw out something like $25 million that is in the right ball park not counting the boosters. Would you agree with this? Dr. Kupperian has been in the budgeting for a long time. Would you care to stick your neck out?

DR. KUPPERIAN: If you don't have any troubles, you probably could.

DR. ROMAN: I think the real answer to this question is that the person who asks it and the others in the audience could probably do a better job of telling us than we can of telling them.

DR. SCHILLING: Any more questions along this general line of managerial arrangements, contractual arrangements?

MR. BAXTER: I don't remember what you said. If we have ideas, whom would we see?

DR. SCHILLING: Whom would you see? Well, as in the past, I want to explain that Dr. Roman is head of our Observational Astronomy Program. This is one of the projects where she has administrative management and responsibility. Participating in this project from the NASA engineering scientists, our Ames Research Center. They have prepared the presentation. Some of you have visited them out there.

Just come and see Dr. Roman. I think your question was really related to the immediate interest for study. Any inquiries you have, please address them to Dr. Roman. If you have project proposals just address them to National Aeronautics and Space Administration. They will all go to our Research and Contract Office; Dr. Lloyd Wood will receive them. They will again end up with Dr. Roman. It is her responsibility and her associates to make decisions.

DR. ROMAN: I should like to repeat what I stated earlier, though, that I think all of you will be hearing from our procurement office, I hope in the not too distant future, which will give a few more details on the process of submitting proposals for study contracts in these areas.

MR. GILBERT (Bendix Corporation): Will these be on a competitive basis, or will these be a sort of solicited list?

DR. SCHILLING: The major procurement items as well as small items, regardless of funds, will be on a competitive basis. In fact, within the Astronomy and Astrophysics Program all the basic research contracts that have been let so far have been competitive. Believe me, we have had hundreds of proposals. However, when we talk about straight procurement items as against basic research aspects, we talk about formal bidding procedures.

DR. ROMAN: I think I would like to restate what you have said in a slightly different way. I am saying the same thing. Any proposals which you submit to us are necessarily going to be judged on a competitive basis. If Bendix submits a proposal and Ryan submits a proposal for the same thing, we are going to look at it and decide, well, who looks like they are going to get the best results and who is going to do it for the least money. I don't think we are at the stage where we can give you a firm set of specifications and go out to the type of bids where we want the lowest price on producing 10 packages with very detailed parts to them.

DR. SCHILLING: Since funding was mentioned, this list which you read, Dr. Roman, this does not talk of millions of dollars?

DR. ROMAN: No.

DR. HELVEY: What kind of ceiling would you say? You said not a million dollars. \$100,000? \$50,000? \$10,000

DR. ROMAN: I ~~think~~ it depends largely on the contract. I think the numbers you have been mentioning are in the right ball park -- not the million dollars.

DR. SCHILLING: In the present fiscal year the funds are very much limited. We are really talking about something like 10 to 50 thousand dollars in one subject area as against the other, which is really something to get started in research development along this line.

Dr. Roman, you have two films which will be shown, I understand.

DR. ROMAN: Would you like to see a film on the air floating platform or not?

DR. SCHILLING: Since also earlier this morning there was mentioned a study being done by Dr. Meinel for the National Science Foundation in basic research aspects of astronomy in space, is Dr. Meinel in the audience?

Perhaps if you could come up here and describe in a few words how this fits in? I want to mention that we have in existence our working group on orbiting astronomical observatories and Dr. Meinel is a member of it. Could you come up and describe in a few minutes what you are doing and how it fits in with the long range plans?

DR. MEINEL: Dr. Schilling, earlier today, in fact starting the presentation, Dr. Roman mentioned two reasons to go above space. All the experiments that have been described involve one of those reasons only. That is the fact that you are above the selective absorption of the atmosphere. The other one, above the turbulence, leads to another aspect that is the high resolution one.

Rockets have gone above the atmosphere, and have explored the ultraviolet section of the sun. Also balloon telescopes have gone high in the atmosphere to gain resolution. As in the case of the rockets, the balloon has only an occasional glimpse. Repeats are slow in between and they serve as, again, rockets to whet the appetite of scientists to know things a little bit more on a continuous basis.

Forgetting the fact that you need a vehicle, we are examining the question of just what would meet both requirements. This has led us to a size, a 50 inch apperture as being the smallest size telescope that would permit a significant gain in angular resolution over earthbound telescopes. This sets a rather large size and involves quite a bit of weight and is out of the question as far as the vehicles that we have been talking about today are concerned.

We also considered what would be the most desirable orbit to put this in and came to the conclusion that the 24 hour orbit has many things to commend it. You suffer quite a payload penalty to get to that altitude, you must admit, but such things as gravitational torque are less by factors of the order of a hundred or more, reducing the disturbance problem from the gravitational torque. Your gain outside the Van Allen zone is roughly the same level you would get in some of the low altitude orbits. You still have the radiation pressure problem to bother you. You are out of the sputtering problem entirely. You have a continuous access problem so that there is no data storage involved. In short, you admit to the payload penalty, you have benefits from there on.

This type of project is -- Dr. Schilling refers to the one discussed today as the second generation for third or fourth generation. Nevertheless, some of the problems appear to have such a magnitude that some study has to be done reasonably early. That is one of the reasons why we are exploring it. As far as vehicle goes, there are vehicles on the horizon such as the Saturn that have the payload capability for 24 hour orbit that is required by this astronomical requirement. In our detailed design we have departed a few places from the philosophy of the present one. One is in our star acquisition and guidance system. We have one advantage which I can show graphically by the globe here.

We are off 6.6 earth radii so the satellite is somewhere in this position with respect to the globe. As a consequence, we can select guide stars which are never occulted by the earth or moon. As a consequence, we can have continuous acquisition and guidance on the stars, and the system that we will envisage actually more of a navigational system which picks two bright stars and operates on them almost continuously.

The method of acquisition is simply that once you have oriented the sun, you can set off a definite angle, as Dr. Code is proposing, and simply pick up the first bright star. Then you can set the second angle to pick up your second star. You triangulate your coordinate system and remain on this. As long as the vehicle has orientation position, you can select your stars and maintain these and observe some 90 per cent of the sky without moving off the stars. So it simplifies some of the guiding problems. Of course, the continuous acquisition if this can be operated means that you can use fairly large dishes, you don't need 60 foot dishesto slew at high angular rates to keep on your target. You can only take account of the small perturbations produced by the equatorial bulge and the moon on such an orbit. We are exploring some of these problems.

The moment you get to something as big as 50 inches there is a fundamental question as to whether you can ever expect to put a telescope of that size in orbit with anything approaching the theoretical resolution which is one of the reasons why we wanted to get up there in the first place. So there are many problems and many steps involved.

This study will go on and explore some of these solutions. I think there is mutual benefit in some of these systems. The point I mentioned to Dr. Triplett about the problem of being able to move in a coordinate system from one object to the other. In our case we have the same limitations, we put the inertial absorber on the navigational system. So you never change your inertial absorbers at all. So you get rid of some of these minor problems. The techniques of setting the telescope are different. You simply measure shaft angles with respect to your navigational system to your main package.

These methods are used conventionally. The question of shaft digitizing is rather standard process so that it involves a different type of acquisition. Of course, the accuracy with which you can fundamentally set the telescope is still limited to perhaps three minutes of arc. But this is usually close enough to pick up the object you wish to study in your prime optical system. So it perhaps has an advantage from that standpoint.

Now, there are undoubtedly another bunch of questions coming up since the radiation torque is not changed. In our preliminary design work we hit a rather dfficult problem. For instance, just simple configuration wise, because the solar cells are essentially black, the rest of your vehicle will probably be white because of thermal problems. If your radiation pressure is balanced on the center of gravity in one orientation, you change it, you shift the center of gravity, and you develop a new torque. It seems, offhand, about the only thing that is really independent of this is a white billiard ball covered with freckles, and the freckles are solarcells. Of course they are not working at maximum efficiency, but it is the only configuration we can see that you have any symmetry.

I think this outlines a few of the things that we have been looking at with the view that some day the capability may exist and need may exist for such an instrument.

DR. SCHILLING: Thank you, Dr. Meinel. May I add hopefully, maybe three years from now when the problems which we heard about this morning are all solved and the vehicle approaches a count down stage, we will have a similar meeting here where we will discuss what to do next.

Are we ready for the film now?

DR. ROMAN: Maybe I can take this time to make one announcement which I was asked to make. It has nothing to do with our program today. Dr. Helvey, of Radiation, Inc., has asked me to call your attention to a symposium on space trajectory which is being sponsored by ARPA and the American Astronautical Society, being held in Orlando, Florida, in mid-December. For details I suggest you see him. You had better stand up so that people will know what you look like.

Are there any other questions while we are waiting?

QUESTION: If by some stroke of luck someone should propose a novel idea for a development program, would that idea be put up for competitive bids, or can a negotiated contract be arranged?

DR. ROMAN: I think I see what you mean. I think the question we were answering before was a little bit different from that. I think at this study contract phase we would consider that the competititon was in the ideas. It does not mean that having been given an idea, we would then take it and send it out to industry for bids.

DR. SCHILLING: Any more questions?

MR. BURNELL (Aeronautical Lab): With respect to this 24 hour orbit, have you considered, or would you care to say something about the problem of keeping the satellite over the part of the earth from which you can see it over a reasonable period of time?

MR. MEINEL: We have considered that and ceased to consider it when we were simply told that they had to achieve this capability for other problems of far greater importance. Therefore, we assumed that it would be achieved. That is an easy way out.

DR. SCHILLING: Dr.Roman, it looks like we might get one of your observatories up before we can get a projectionist.

DR. ROMAN: Mr. Gillespie, from the Langley Research Center. He owns one of the films which we are waiting to see.

MR. GILLESPIE: The Space Orientation Control Programs at the Langley Research Center, I believe in our preliminary work have come up with two results which may have some application in this particular space mission. Item No. 1, we have been working on a design for a solar sensor which makes use of silicon cells mounted at an angle which would be oriented to the direction of the sun. These would be connected in a bridge circuit and their output of course then would be working against each other to give a control signal. The configuration we have drawn or pictured on the first slide.

(Slide)

I don't know if this is visible to the people in the back. On the left side we show what we call some coarse sensors in two arrangements, one with an indented or inverted pyramid around triangular cells which in this case would have a capture capability of approximately 90 degrees. In the bottom case we have raised these cells up above the surface of the vehicle which in this case is not a flat surface, but which continues the slope of the silicon cells and increases the angle of capture to approximately 150 degrees. For this case the cells are mounted 60 degrees from what would be the normal flat surface. Over on the other side we show that by adding a shield, the shield is a long rectangular column, we can increase the sensitivity of this arrangement by the shadow effect from the shield.

For long duration operation in orbit we have indicated a possible scheme for correcting any aging effects which would tend to change the characteristics of the individual components. However, we are uncertain as to how much change may occur and just how big the problem might be.

In the next slide we show the measurements made of the voltage output of the coarse and fine sensors which we described. In this particular case the source of light was an aircraft landing light of rather low intensity compared to the sun, and each cell was really made up of six individual solar cells, which is the reason for the rather large voltage output. But this large voltage output is one reason for going to the silicon cells. That is, we feel there would be less amplification required in that actual final design.

The curve in red here would be for the coarse sensors; by adding the shield we can improve the sensitivity by the shield. I think that will be enough for the slides.

DR. ROMAN: Do we have a projectionist for the movie now?

MR. GILLESPIE: I think I could probably sum up what the movie indicates. We used a preliminary version of this solar sensor in connection with the simple solenoid operated air jet system, cold nitrogen, and the pointing accuracy achieved to date has been plus or minus 18 seconds of arc. To do better than this in the ground simulation test made with the use of the hemispherical air bearing would seem to require some attention to reducing the traditional background noise from the room. People walking around the room and air currents and the like. But we feel that this solar sensor can be developed along with a more refined control system to get down to the one second of arc pointing accuracy which has been mentioned earlier.

The other thing which the film shows is one of two methods we have been looking into for means of unloading the flywheel to prevent saturation. Both methods that we are considering would make use of the reaction we can obtain with the earth's magnetic field. In the film we have made tests with a combination system of a flywheel working along with a bar magnet, with the bar magnet giving a capability for reducing the flywheel speed.

A second alternate method which we have not yet tested but which looks feasible for the case where the inertials of the satellite are closely equal -- instead of designing the flywheel in the form of an elongated cylinder, to maximize the magnetic skin damping effect that can be obtained by a spinning body in the earth's magnetic field. This seems like a possible solution.

I think if we have the film, we can then start. The film has titles which are somewhat self explanatory.

DR. ROMAN: I would like to suggest that since there has been the difficulty and the delay in the film and I know that at least some of you have other appointments to catch, that perhaps we should adjourn the meeting, and then those of you who would like to stay to see the two movies are perfectly welcome to do so.

Before I adjourn I have a message here for Mr. Reichenbach of Kearfott Company, if he will see me. I want to thank you all for coming. I am very sorry I kept you waiting this afternoon, but I appreciate your loyalty in waiting. Back at college it used to be you waited ten minutes for a full professor, and then you left. So I was rather startled to see a room full of people here when I returned. However, thank you, and I imagine I will be seeing you again. I know you will be hearing from me.

(Thereupon at 3:15 p.m., the meeting was concluded.)

Figure I - Milligan - Goddard Space Flight Center

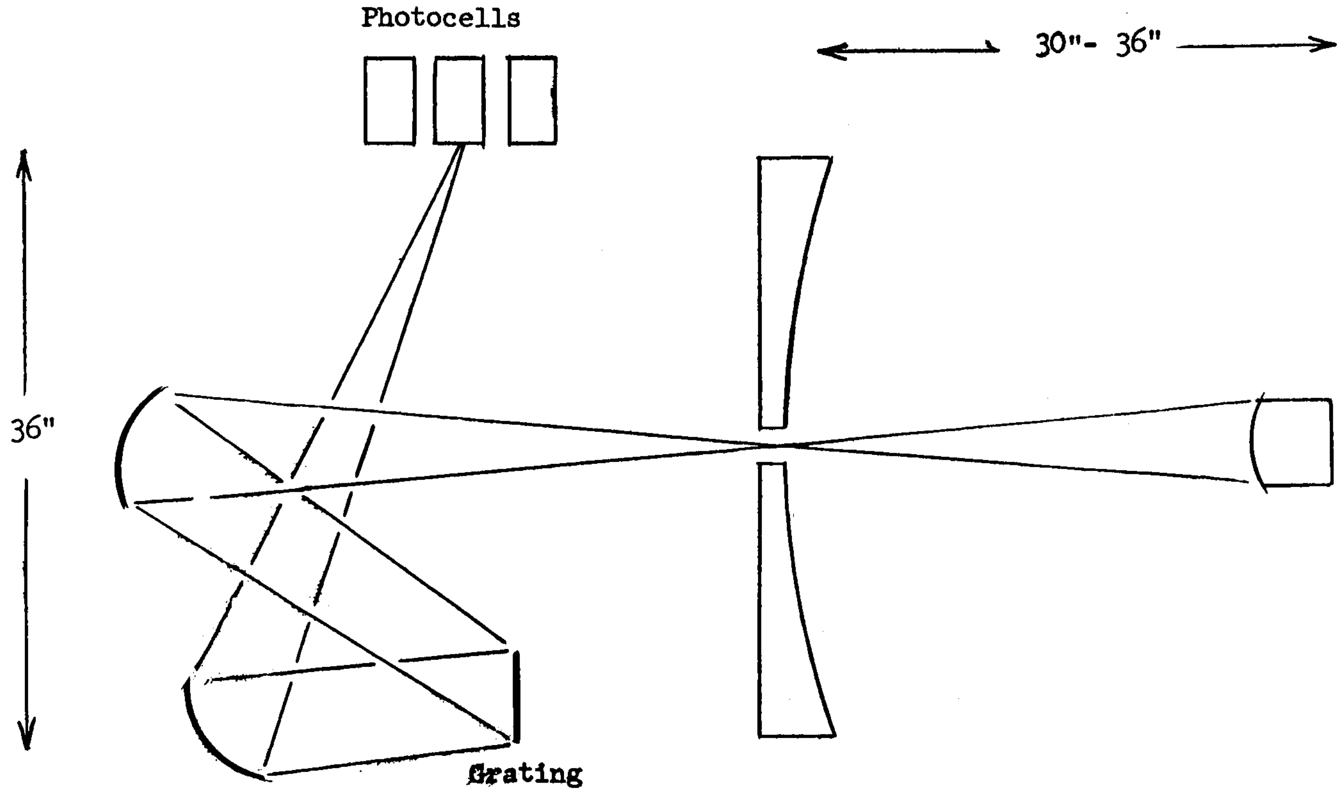

Figure 2 - Rodgerson - Princeton University